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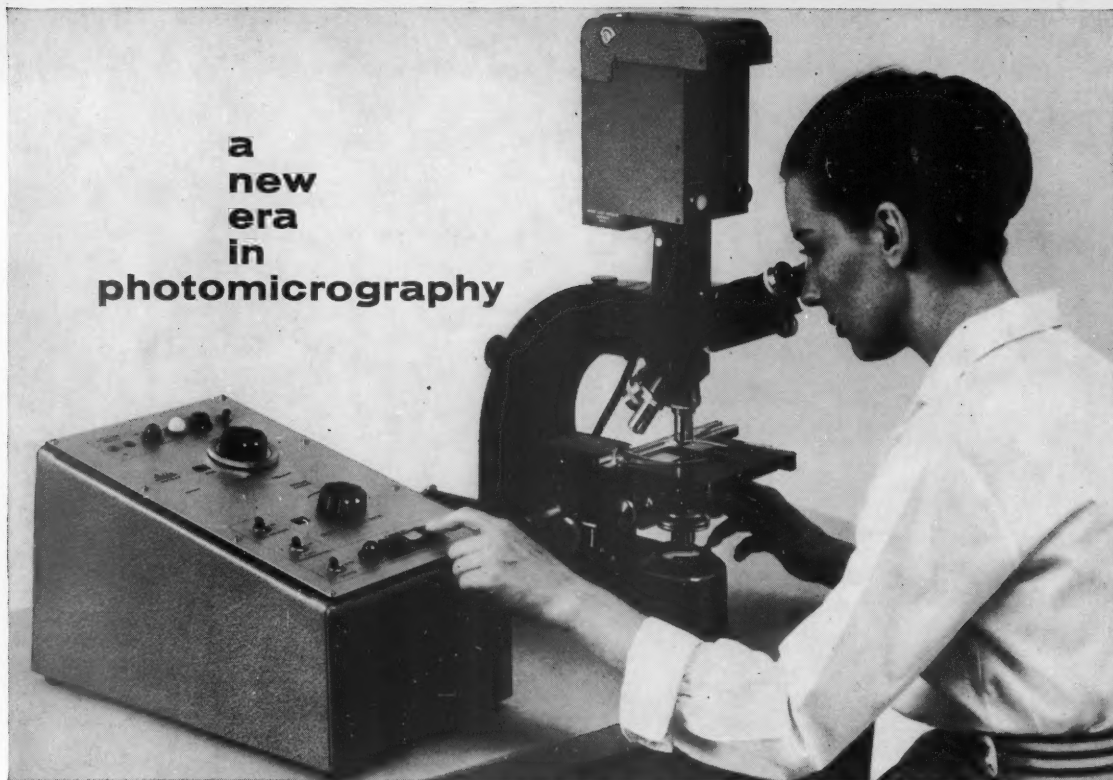
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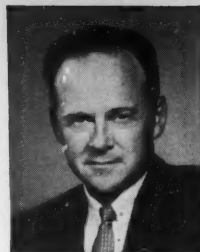


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## Basic Research at Honeywell

Dr. Finn Larsen

Vice President for Research



# The Nature of Oxidation: Studies In The High-Temperature Oxidation of Alloys

Under high temperatures, oxidation is accelerated. While some pure metals deteriorate rapidly, certain of their alloys oxidize much more slowly. Accurate prediction of alloy oxidation rates, however, awaits development of a reliable mathematical model. At Honeywell Research, new techniques have produced data that make a start toward a universally applicable theory.

With the single exception of gold, oxidation limits the use of all metals at high temperatures. This is true because the products of corrosion do not have the properties of the parent metal. In addition, corrosive products occupy more space than the parent metal they replace, affecting dimensions and tolerances.

Corrosion is greatly accelerated by high temperatures, putting serious limitations on progress in heat generating equipment such as internal combustion engines, rockets, nuclear reactors and electrical contacts.

At the present time the accepted method of inhibiting corrosion is to apply a protective coating to the metal to prevent the migration of oxygen atoms to the surface of the material. This, however, is expensive and in many cases not practical.

We know that when an oxide free surface is exposed to ordinary air at room temperature the upper layers of the metal combine with the oxygen atoms to form a thin film or scale (oxide). For further oxidation to occur the thin oxide film must be penetrated by either oxygen atoms migrating down to the fresh metal surface or by metal atoms migrating outward to the air. In most cases, one of these reactions predominates.

For about 40 years metallurgists have worked with several classical equations that predict the rate of oxidation. However, these equations apply rigorously only under idealized conditions. They do not fully equate the mechanical and microstructural features of a multi-layer oxide or the dislocations and stresses that affect the oxidation process. For example: Is the oxide film ductile or brittle? A change of temperature puts thermal stress on the oxide and if it is brittle it will probably break off. These properties modify the

classical theory. All of these problems multiply and each influence is changed when an alloy is introduced.

Honeywell scientists hope to learn more about these altering influences in order to extend the classical equations. They are analyzing multi-layer oxide scales with a number of different laboratory methods to build support for new, predictable behavior.

Multi-layer scales are caused by the ability of metals to have multiple valences. The balance between these layers is controlled by temperature. When a multi-layer scale exists, oxides are often unable to relax the stresses that occur. These stresses are caused by the differences in specific volume and the differences in thermal coefficients of expansion between the oxide and the metal. When they cannot be relaxed, stresses may build up and affect the rate of oxidation. Also, if external stresses are applied to the material the rate of oxidation may be affected.

The approach to this study quickly becomes a mixture of metallurgy and physical chemistry. One technique in studying rate of growth of the scale has been to measure the weight gain of alloys during oxidation. Reliable data on oxidation has been obtained in this manner.

To determine the direction of the migration of ions and also measure the growth of individual layers, Honeywell scientists are welding thin platinum wires to a specimen prior to heating. These marker wires give a point of reference to the original surface. If oxygen ions are moving inward, the wire remains outside the surface. If cations are moving outward, the marker wire will be under the surface. This method has yielded valuable new information on the formation of oxides.

Microscopic examination also has been helpful in identifying layers, and X-ray diffraction has given positive identification of the oxide phases.

Ideally we would like to completely inhibit even the first monatomic oxide layer. At the present state of knowledge, this seems unattainable. Our approach then is to utilize the natural oxidation process but control it. By doing this we permit the formation of a thin film but seek to make it impermeable to further ion migration.

In our experiments Honeywell scientists have effected radical changes in oxidation rates by changing the oxide microstructure through heat treatment of its alloy. For example, with an alloy of .87 Mg—.62 Cu, the oxidation rate can be retarded and the resulting oxidation reduced by a factor of ten with proper heat treatment.

We now know that in a polycrystalline structure, stress and mechanical properties affect both the rate and the mechanism of oxidation. Also we know that the mechanical properties of the oxide have a decisive effect on the tendency of the oxide to either spall or adhere.

This is a start toward the derivation of a general theory explaining the oxidation of alloys. Though our research is basic at this point in time, we expect it to yield many practical answers to assist the design engineers working on high temperature problems confronting today's nuclear and space projects.

If you are engaged in scientific work relating to oxidation of metals and would like to know more about Honeywell's research on this subject, you are invited to correspond with Dr. J. A. Sartell, Honeywell Research Center, Hopkins, Minnesota.

If you wish a recent paper, "The Role of Oxide Plasticity in the Oxidation Mechanism of Pure Copper," by Dr. Sartell, write to Honeywell Research, Minneapolis 8, Minnesota.

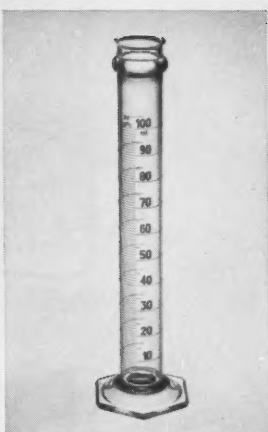
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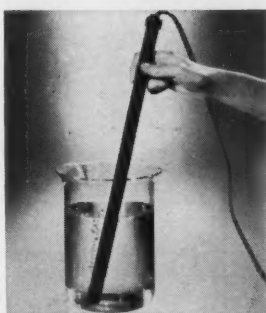
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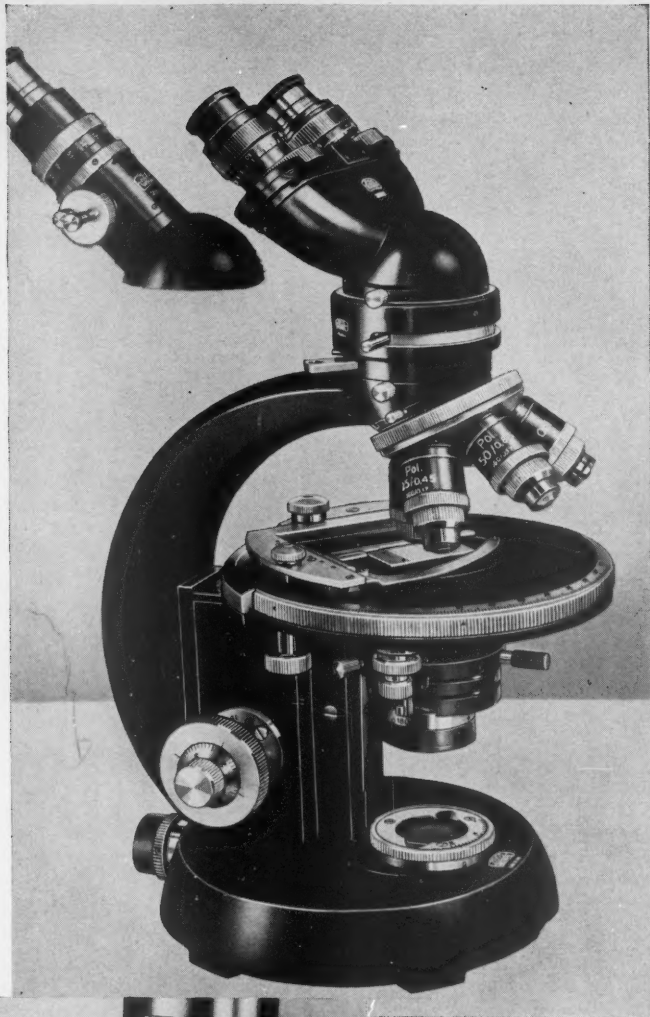
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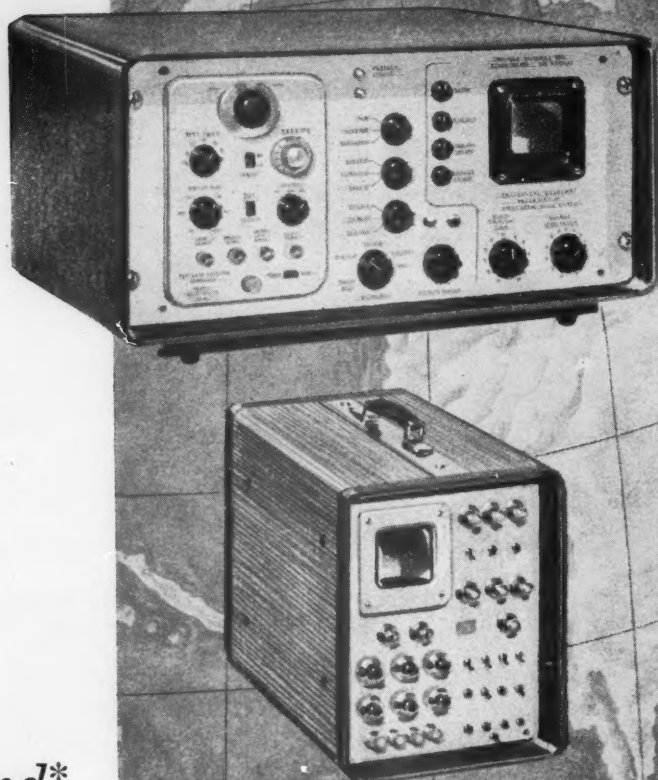
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## The Stowe Conferences on Science and World Affairs

The resumption of nuclear weapons testing, announced just before the Stowe Conferences opened, immediately made it doubtful that the conferences could be held at all. It is consequently all the more significant that the participants were able, in spite of a stormy atmosphere, to arrive at constructive recommendations (see page 984). As befits scientists, views from East and West were exchanged with frankness and reasonable objectivity.

The first of the two conferences, being the less controversial, more quickly attained agreement on certain measures. If carried through, the resulting international cooperation in science ought to help lessen international tensions. With the example of the I.G.Y. before us, one may readily hope for constructive action in such joint enterprises as mapping the ocean floor and surveying the changing waters and life of the seas; drilling through the earth's crust; forecasting weather and natural catastrophes on a world-wide basis; increasing the fresh-water resources of the lands; developing food resources and farming the oceans; exploring space and internationalizing the moon; using satellites for communications systems; preserving and promoting health; grappling with human pollution and waste of the natural environment; exploring molecular biology; building a 300 billion electron volt accelerator and the world's greatest computer; and establishing in a strategic place—some persons propose in Berlin—a great international cluster of science laboratories and institutes. Nor was scientific aid to the less-developed countries forgotten, or the value of greatly extended and freer exchanges of scientific personnel and information overlooked.

Nevertheless, in the fine glow of such hopes, every mind harbored the unspoken recognition that none of these joint efforts could germinate in a world on the eve of nuclear war. The success of the Eighth Conference, on the subject of Disarmament and World Security, hence reflected the world's desire for an ultimate resolution of tensions and enmity. For the participants, it was not easy to speak without mutual recrimination or anger. Nor does the final public statement express much more than a common hope for peace.

All the more remarkable, then, is the fact that at least three working groups, each including leading experts from East and West, found it possible to agree upon measures for the cessation of the production of fissionable materials for military purposes and the elimination of stockpiles of nuclear weapons; for the similar and parallel elimination of long-range missiles, bombers, submarines, and other means of delivery; for staging the first phase of disarmament so that inspection and control can increase as disarmament progresses, and so that each side may retain security in the process. A hard look at the kind of world that would exist after complete and general disarmament clearly indicated the need for an international police force and a system for the settlement of disputes between countries, in order to prevent rearmament and to permit peaceful accommodation.

These working papers, which so far exceed in extent of agreement and explicitness the former bases of negotiations at Geneva, may perhaps, both sides willing, bring us closer to the ultimate goal declared by the leaders of the world to be the hope of every nation, the banishment of war.—BENTLEY GLASS, *Johns Hopkins University*





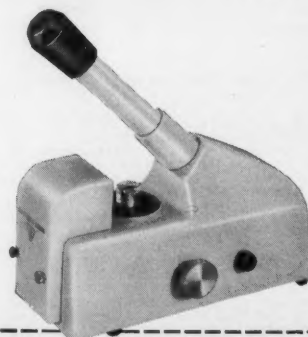
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## Lifetimes of Orbiting Dipoles

Sunlight pressure should limit the average orbital lifetime of West Ford dipoles to about 7 years.

Irwin I. Shapiro and Harrison M. Jones

The West Ford experiment (1) is concerned with creating a belt of orbiting dipoles for study by radio-wave propagation and communication scientists. Some 75 pounds of tiny hairlike copper dipoles are to be injected into a circular polar orbit about 3800 km above the earth's surface; the resonant frequency of the dipoles will be near 8000 Mcy/sec. Radio-frequency equipment located near San Francisco and Boston will be used to conduct communications tests and to study the physical and electrical characteristics of the belt by means of monostatic and bistatic radar experiments.

At various times, apprehension has been expressed concerning several possible deleterious effects which might result from such a dipole belt. Published quantitative investigation (2, 3) of these possible effects for this experimental belt show such concern to be groundless, since the individual dipoles are small and light, and since the total amount of material is modest. Because of the dispersion of the belt, even these small effects will be reduced by about an order of magnitude in a few years.

However, to allay concern over possible interference with scientific measurements which might be made perhaps decades in the future, the experimental dipole belt is to be formed in an orbit with a relatively short lifetime.

Since the belt must be placed at a high altitude, the cumulative effect of atmospheric drag cannot be counted upon to remove the belt from orbit in a reasonably short time. However, by using dipoles with a high area-to-mass ratio and by choosing a resonant orbit, solar radiation pressure can be utilized to drive the belt downward into the dense regions of the earth's atmosphere. The influence of sunlight pressure on orbiting objects of such high area-to-mass ratio has been the subject of considerable analytical study (4, 5), and detailed quantitative verification has been obtained from observations of Echo I (5).

When producing a desirably short belt lifetime, sunlight pressure also has the undesirable effect of causing the belt to disperse because of the random tumbling patterns of the dipoles. Generally speaking, the smaller the dispersion of the belt after a given period of time, the longer will be its ultimate lifetime. Since the propagation and communication studies require an adequate period during which the belt will remain relatively undispersed, a compromise in lifetime must be made. The values chosen for the West Ford experiment appear to meet both lifetime criteria reasonably well: dipoles with maximum cross-sectional area-to-mass ratios of about  $55 \text{ cm}^2/\text{g}$  injected into circular polar orbits at 3800-km altitude near the time of the winter solstice can be expected to have an average orbital life-

time of about 7 years and a useful experimental lifetime of about 2 years.

In the remainder of this article we give a general description of sunlight pressure perturbations with particular emphasis on resonant orbits, and we explain in some detail the methods used and the results obtained in calculations for the West Ford dipole belt.

### Sunlight Pressure Perturbations

The effects of sunlight pressure will be of paramount importance on the orbits of earth satellites with high area-to-mass ratios (like the West Ford dipoles). Because of widespread unfamiliarity with this subject, we present a qualitative description of these effects and then explain the concept of resonant and nonresonant orbits. For most of the discussion we assume that the sun lies in the plane of the satellite's orbit; however, our arguments will apply equally well for the general case. The only difference is that our statements should then be interpreted as referring to the component of the radiation force lying in the orbit plane. (The component of the force perpendicular to this plane has a much smaller effect on the orbit.)

Figure 1 illustrates the action of the sun's rays on an initially circular orbit. While the radiation exerts pressure continuously (except when the satellite passes through the earth's shadow), we consider its effects only at the two points *A* and *B*. At point *A*, the satellite is moving away from the sun; hence the radiation pressure causes a slight increase in the velocity of the satellite which, at the same time, increases the orbital energy of the satellite and therefore its major axis. The result would be an orbit like the dotted one passing through *A*. On the other hand, when the satellite is near point *B*, the sun's rays push against the direction of motion of the satellite, thus slowing it somewhat. This causes a decrease in the major axis, and tends to produce an orbit like the dotted one passing through *B*. Similarly, one can calculate the effect

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of sunlight pressure on all points in the orbit; the result is simply a displacement of the entire orbit in a direction perpendicular to that of the radiation force (as indicated in Fig. 1). The magnitude of this displacement is directly proportional to the area-to-mass ratio of the satellite.

If the earth and sun were stationary with respect to one another, and if no other perturbing forces acted on the system, then the radiation pressure would cause a monotonic decrease in the perigee height of an initially circular orbit, until, finally, the satellite collided with the surface of the earth. This simple conclusion is invalidated mainly by the two following facts:

1) The earth rotates about the sun, causing the direction of sunlight pressure on the satellite to change, and the sunlight therefore to push the orbit in a new direction. Instead of a monotonic decrease in perigee height, one now finds that perigee height oscillates. After 6 months, for example, the sun's rays

will push in the opposite direction, causing a displacement of the orbit opposite to the original one.

2) The earth's equatorial bulge causes the orbit to rotate in its plane. Again, since sunlight pressure displaces the orbit perpendicular to the earth-sun line, perigee height will oscillate rather than decrease monotonically. For example, when (because of the rotation of the orbit induced by the bulge) the position of perigee has rotated  $180^\circ$ , the displacement of the orbit due to solar pressure will increase the perigee height instead of decreasing it as originally.

For certain orbits (which we call resonant orbits of the first type) the effects mentioned separately in the two preceding paragraphs will cancel each other. That is, the rotation of the orbit in its plane (due to the earth's bulge) will just compensate for the change in direction of the sun's rays (because of the earth's rotation about the sun). In such cases the simple situation described

earlier will again obtain: The perigee height of initially circular orbits will decrease monotonically, thus enabling us to predict lifetimes with reasonable accuracy irrespective of our inadequate knowledge of air densities. For non-circular initial orbits the perigee height may first increase and then decrease monotonically, depending on the initial orientation of the apsidal line with respect to the sun's rays (6). The lifetimes of satellites in these resonant orbits are inversely proportional to their area-to-mass ratios.

For polar and near-polar orbits it is also possible to have a somewhat different type of resonance. For this (second) type, the rotation of the orbit in its plane (due to the earth's bulge) is equal in magnitude but opposite in direction to the rotation of the component of the earth-sun line in the orbit plane. Figure 2 illustrates such a resonance; it is characterized by seasonal, small amplitude reversals in direction of the changes in perigee height (7). The second type is obtained from the first by a reversal in the direction of satellite motion. The most pronounced difference between the two occurs for the pair of ascending node ( $\Omega$ ) values  $0^\circ$  (type 2) and  $180^\circ$  (type 1). As  $\Omega$  increases by  $90^\circ$  the distinction between the types gradually disappears.

What orbits, one might ask, will be resonant? Since the rate of rotation of the earth about the sun is fixed, we can only adjust the rotation rate of the satellite orbit in its plane by suitably choosing the initial orbit. This latter rate depends on the satellite inclination angle, on its major axis, and (weakly) on its eccentricity. The explicit formulas for the mean rotation rate of the orbit in its plane due to the bulge ( $\dot{\omega}_b$ ), and for the mean rotation rate of the plane itself ( $\dot{\Omega}_b$ ) were given in a previous publication (4). For a polar orbit we find

$$\dot{\Omega}_b \approx 0; \dot{\omega}_b \approx -\frac{5}{a^{7/2}(1-e^2)^2} \text{ deg/day}, \quad (1)$$

where  $a$  (the semimajor axis) is expressed in earth radii.

If we restrict ourselves to initially circular polar orbits, there is only one value of  $a$  for which  $\dot{\omega}_b$  will equal the rate of rotation of the earth about the sun ( $\dot{\beta}$ ). The altitude corresponding to this  $a$  is 3700 km. Actually, there exists a small band of initial orbits near this mean altitude which behave like resonant orbits. The bandwidth depends on the area-to-mass ratio ( $A/M$ ) of the satellite; in general, it increases with in-

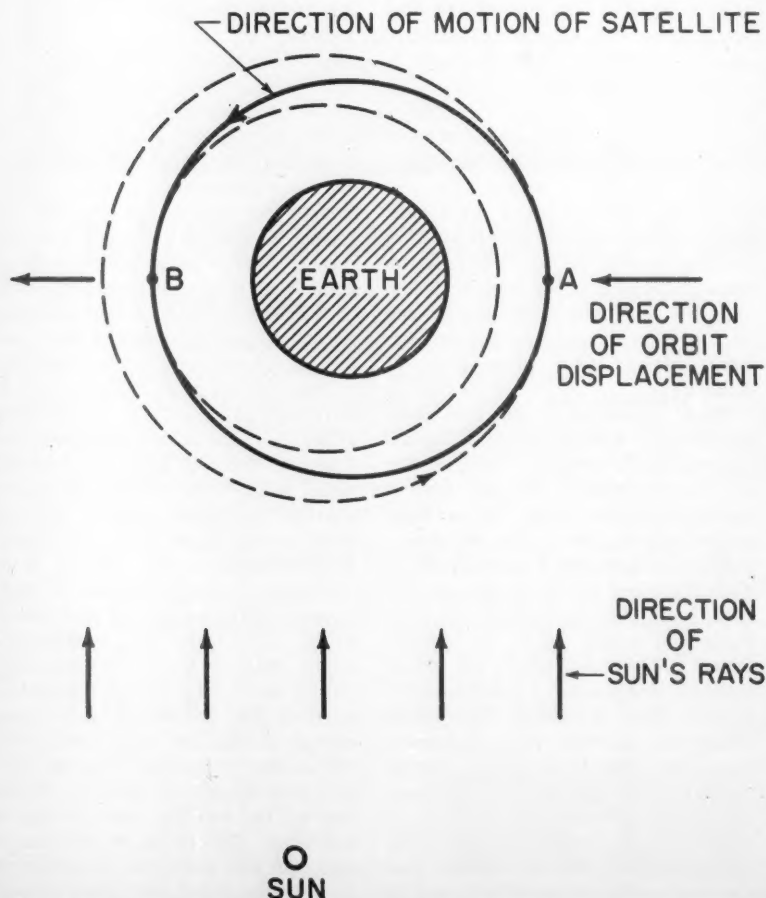


Fig. 1. Displacement of a satellite orbit by sunlight pressure.

creasing  $A/M$ . Thus, for an  $A/M$  value of  $50 \text{ cm}^2/\text{g}$ , it is approximately 300 km, whereas with  $A/M = 250 \text{ cm}^2/\text{g}$  the width is about 5000 km.

This band is, however, *not* centered at a 3700-km altitude: Since  $e$  increases monotonically in a resonant orbit, we see from Eq. 1 that the magnitude of  $\dot{\omega}_b$  will also increase monotonically (8). Hence, the magnitude of  $\dot{\omega}_b$  will not remain equal to  $\dot{\beta}$  and the resonance condition will not continue to hold precisely. If an initial altitude somewhat greater than 3700 km is chosen, then the value of  $\dot{\omega}_b$  (averaged over the lifetime of the satellite) will be closer in magnitude to  $\dot{\beta}$ . For this reason, the band of initial polar orbits, each of which behaves most like a truly resonant orbit, is centered at an altitude above 3700 km.

For all mean altitudes that lie outside the narrow resonance band, we see that  $\dot{\omega}_b$  will be noticeably different from  $\dot{\beta}$ . Hence, the effects 1 and 2, mentioned above, will not cancel, and perigee height will oscillate. The amplitude

of this oscillation is (roughly) inversely proportional to the difference in the magnitudes of  $\dot{\beta}$  and  $\dot{\omega}_b$ . It is, of course, also proportional to the value of  $A/M$ .

### Effective Area of a Dipole

From the above discussion, we see that the lifetime of a West Ford dipole (that is, the time span during which it remains in orbit) is critically dependent on the initial orbital altitude and inclination angle. This lifetime also depends on the value of  $A/M$  appropriate for the dipoles. More specifically, the area of a dipole when projected on a plane perpendicular to the earth-sun line is the relevant value of  $A$ , the calculation of which is complicated by the tumbling of the dipoles: Theoretical and experimental studies conducted under conditions closely simulating those expected to be encountered in space indicate that about 95 percent of the dipoles will be dispensed with tumbling rates exceeding 2 rev/sec (9). The tumbling periods of

the dipoles, compared with their orbital periods, will obviously be quite small. In calculating an effective projected area, we therefore average over a tumbling period. Simple geometrical considerations show that the maximum  $A$  ( $A_{\max}$ ) results when the angular momentum vector (10) of the dipole is oriented along the earth-sun line; parallel reasoning indicates that the minimum value of  $A$  ( $A_{\min}$ ) occurs when this vector has a perpendicular orientation. The value  $A_{\max}$  is given by the product of the dipole diameter and length;  $A_{\min} = (2/\pi) A_{\max}$ . (For simplicity, we are restricting this discussion to dipoles which absorb sunlight and re-radiate thermally. The lifetime results for specularly reflecting dipoles are not appreciably different.)

The effective projected area ( $A_{\text{eff}}$ ) of a given dipole will not remain constant during its lifetime: First,  $A_{\text{eff}}$  will change because of the rotation of the earth-sun line. Second, there are various torques (such as that due to the inhomogeneity of the earth's gravita-

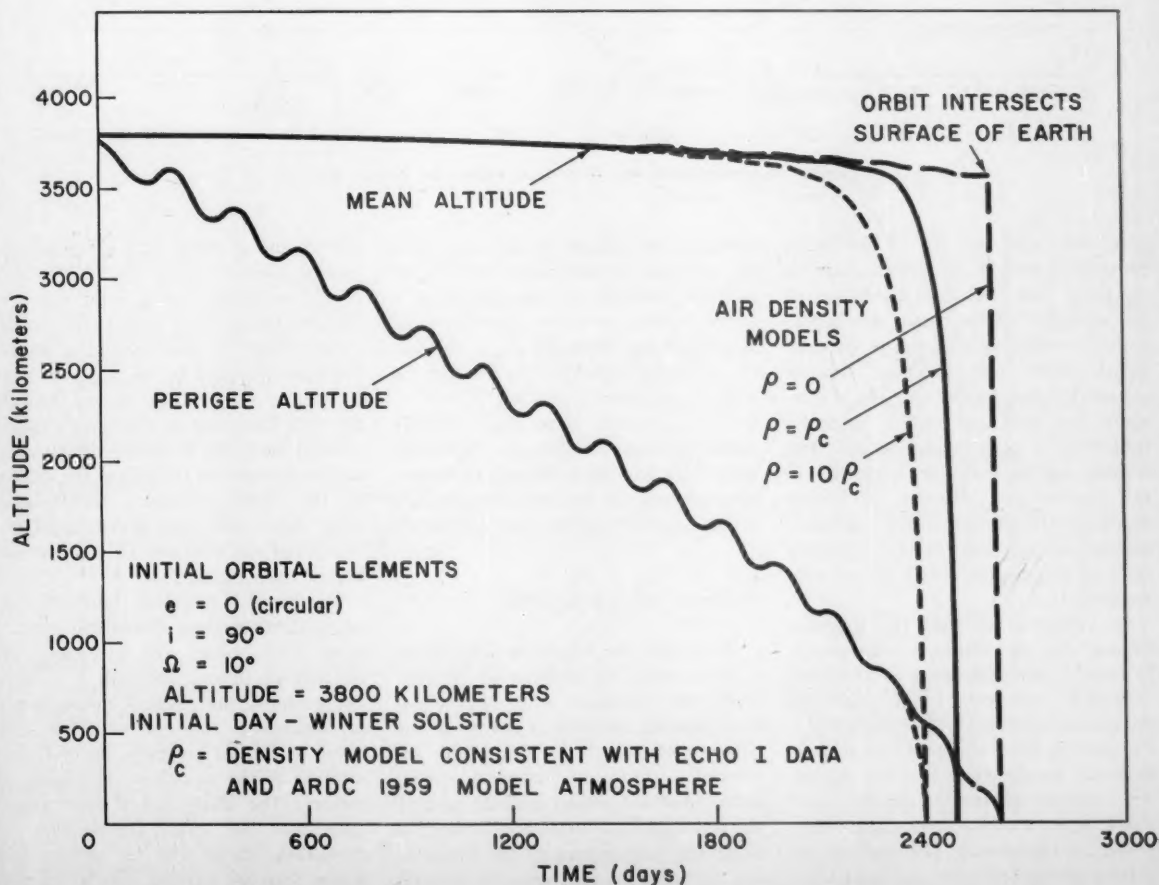


Fig. 2. Perigee altitude and mean altitude versus time for a resonant orbit.

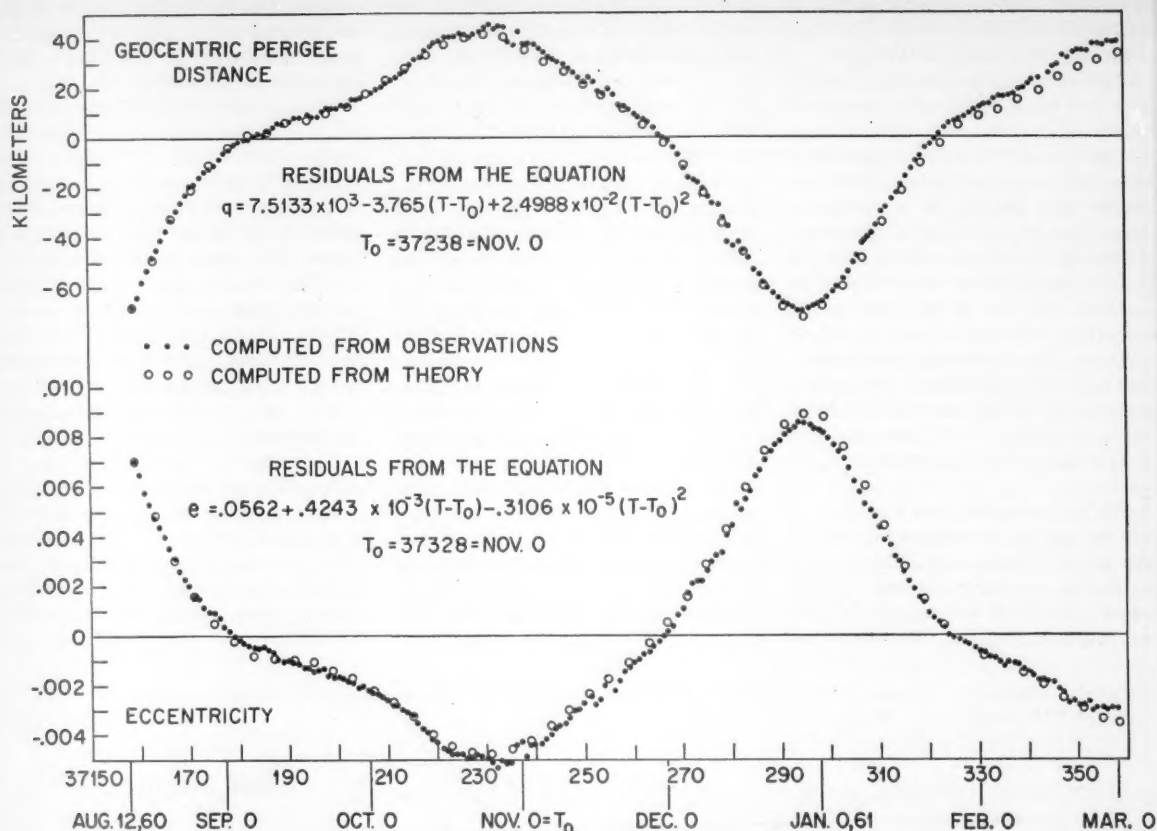


Fig. 3. Comparison of experimental and theoretical values for orbital elements of Echo I.

tional field and that due to the action of solar radiation on irregularities in the shape and/or surface conditions of the dipoles) which cause the dipole angular momentum vector to precess. Third, there are damping torques caused by interaction with the atmosphere and with the earth's magnetic field (that is, eddy current torques and torques due to magnetic impurities in the dipoles). At altitudes of several thousand kilometers these damping torques are so small that the tumbling rates of the dipoles would be virtually unaffected.

In addition, we find that impulse torques due to collisions with micrometeoroids are significant in changing (actually, statistically increasing) the angular momentum of the dipoles (11). Considering all of these factors in combination, we conclude that the dipoles will continue to tumble rapidly compared with an orbital period until they penetrate the denser portions of the earth's atmosphere. For any individual dipole,  $A_{\text{eff}}$  will vary between  $A_{\text{max}}$

and  $A_{\text{min}}$  throughout its life; and, since the lifetime is determined mainly by sunlight pressure, we can determine a lower bound on it by assuming that  $A_{\text{eff}}$  maintains the value  $A_{\text{max}}$ . Similarly, an upper bound on the lifetime for a given initial orbit can be obtained by fixing  $A_{\text{eff}}$  at the value  $A_{\text{min}}$ . (Strictly speaking, these bounds are rigorously valid only for resonant and near-resonant orbits of the first type; the situation is more complicated for a general orbit.)

#### Prediction of Dipole Orbits

To predict the long-term behavior of a dipole orbit we proceed as follows: Using the standard differential equations relating the rates of change of the orbital elements to the perturbing accelerations (12), we integrate analytically over an orbital period, keeping the elements constant during the integration. The new values of the elements are then used to determine the changes in the elements during the next orbital

period, and so forth. This iteration procedure, carried out by an I.B.M. 7090 digital computer, yields good approximations for the long-period and secular contributions to the element changes. We have included in the program perturbing forces due to the second through the fifth harmonic of the earth's gravitational field, the action of direct solar radiation pressure (including the effects of the earth's shadow), atmospheric drag, lunar and solar gravitational effects, and the pressure of sunlight reflected from the earth.

Two types of error are, unfortunately, unavoidable in these theoretical predictions. First, errors are introduced by deficiencies in our theoretical model: For example, the physical constants associated with any perturbation can never be known precisely. Second, our solution to the equations of motion is inexact: The mathematical approximations used will cause inaccuracies in prediction which will be greater the longer the time interval over which the predictions are made.



## Comparison with Orbit of Echo I

To check the adequacy of our theoretical method, we compared our results for the orbit of Echo I (area-to-mass ratio approximately  $100 \text{ cm}^2/\text{g}$ , mean altitude 1600 km, inclination  $47^\circ$ ) with those determined by the Smithsonian Astrophysical Observatory from Baker-Nunn photographic data. Using the average air density as determined experimentally, and allowing the area-to-mass ratio to increase (to take into account in a "best fit" manner the change in reflection properties and the 20-percent loss of mass due to the gradual escape of sublimating powders through punctures in the balloon), we can match the experimental values of all the orbital elements quite well from the time of launch to the present. As an illustration of the good agreement obtained between our theoretical determination and the observed orbit, we reproduce in Fig. 3 a graph from an earlier paper (5) which shows a comparison for

the time variations of eccentricity and of geocentric perigee distance for the Echo satellite from time of launch through 1 March 1961. (To enlarge the scale, both the experimental and theoretical values were plotted as residuals from a polynomial expression obtained by the method of least squares.) It should be emphasized that in our calculations we made use only of the initial (observed) values of the orbital elements; all subsequent values were calculated theoretically in the above-described manner. These results give us confidence that our theoretical procedure is reasonably accurate over long time intervals and that we have not neglected any important perturbations.

## Influence of Air Density

It might be thought that an accurate knowledge of air densities is essential to an accurate prediction of the dipole belt's lifetime. And, in general, this

is true. However, if the belt is in a resonant (or near-resonant) orbit, its lifetime is almost independent of air density since in these cases radiation pressure alone is responsible for driving the orbit perigee toward the earth's surface. In Fig. 2 this insensitivity is illustrated for a resonant polar orbit of the second type. Here we have chosen three different air density models. In the first, the density ( $\rho$ ) is consistent with the data determined from the orbit of Echo I and with the 1959 ARDC Model Atmosphere; in the second,  $\rho$  is everywhere 10 times greater than it is in the first model; and in the third, the density is set equal to 0. We see that these extreme models yield lifetimes differing by less than 10 percent.

For a belt in an orbit sufficiently far removed from a resonance, radiation pressure causes long-period oscillations in perigee height with an amplitude proportional to the area-to-mass ratio of the satellite. For such orbits, the lifetime does depend significantly on air

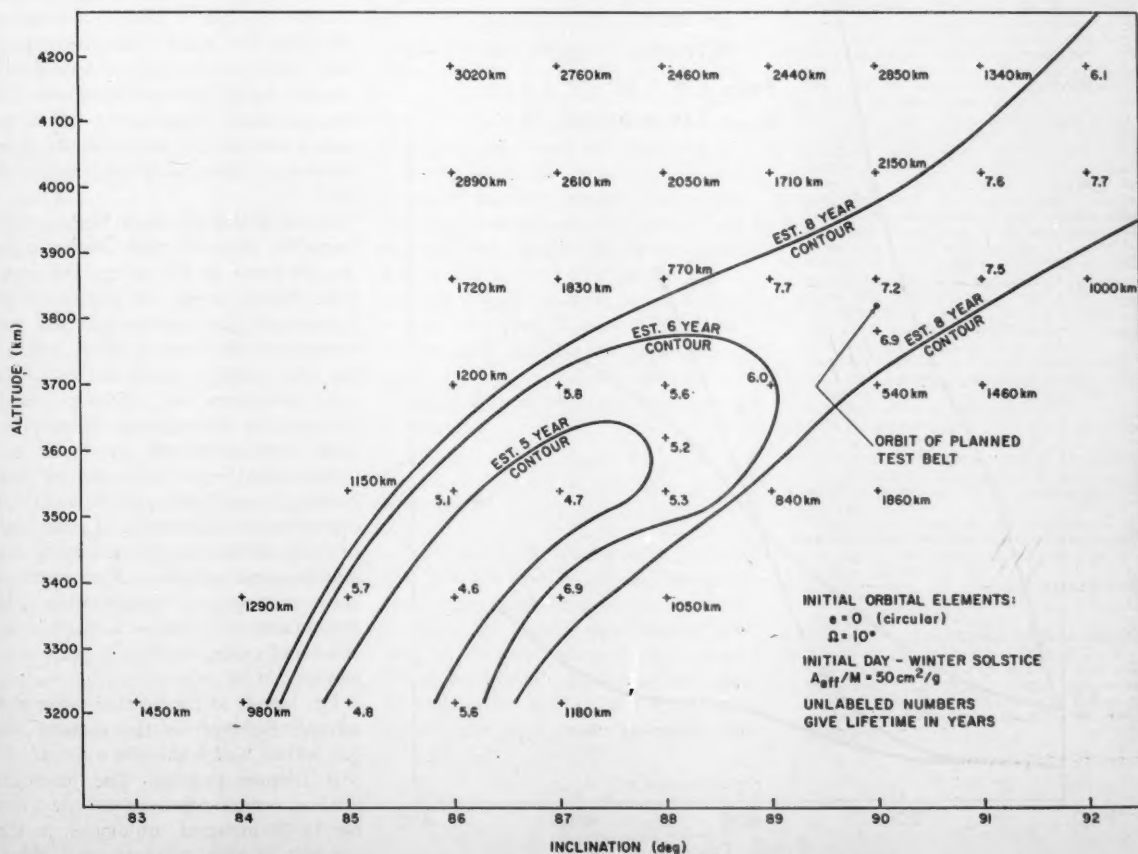


Fig. 4. Lifetime contours for resonant and near-resonant orbits in the polar region.

density. We can make estimates of the effect of density on lifetime for these nonresonant high-altitude orbits by using the following formula:

$$\Delta a = -2\pi C_D (A/M) \bar{\rho} a^2 \quad (2)$$

Here,  $\Delta a$  is the decrease in mean altitude per revolution,  $a$  is the semimajor axis of the orbit,  $C_D$  is the drag coefficient of the satellite,  $A/M$  is an average cross-sectional area-to-mass ratio appropriate for air drag, and  $\rho$  is the air density. As an example of the use of this formula, consider a dipole orbit whose perigee altitude is in the vicinity of 1600 km. From the Echo data, it has been established that a representative air density at 1600 km is approximately  $10^{-18}$  g/cm<sup>3</sup> (5, 13). Therefore, by assuming  $C_D$  to be 5 (to take into account in an approximate manner both

charge and neutral drag), we see that the mean altitude will decrease at a rate of less than 50 km/yr. When the oscillations caused by sunlight pressure bring the orbit perigee to altitudes lower than 1600 km, similar estimates can be made for the rate of decrease of mean altitude in these regions. Crude estimates of the lifetime of dipoles in clearly nonresonant orbits can be constructed by this method. The minimum air density likely to be found at altitudes of several thousand kilometers is that of interplanetary space, which is estimated to be several orders of magnitude less than  $10^{-18}$  g/cm<sup>3</sup>. Using this smallest possible air density, we can obtain estimates which are rigorous upper bounds on belt lifetimes. (These will be quite far above least upper bounds, even for nonresonant orbits.)

## Orbital Lifetime

As stated earlier, the West Ford test belt will be placed in a circular polar orbit at an altitude close to 3800 km; the dipoles to be used have a value of  $A_{\max}/M$  of about 55 cm<sup>2</sup>/g and hence a value of  $A_{\min}/M$  of about 35 cm<sup>2</sup>/g. To illustrate the lifetimes of dipole belts in this region we have constructed the contour curves shown in Fig. 4. Before discussing these results in detail, we note that it is impossible to present succinctly the detailed dependence of lifetime on all the relevant parameters. Therefore we are concerned mainly with showing the behavior of lifetime with respect to the most critical parameters, altitude and inclination angle. Thus all the data presented refer to initially circular orbits, for the test belt is expected to be nearly circular and our results for lifetime are insensitive to small changes in eccentricity.

For the planned West Ford belt (and hence for all of Fig. 4) we have chosen the winter solstice as a representative initial day and have assumed that the launch will occur at dawn in a southerly direction. The initial polar resonance is then of the second type, and yields the greatest dipole lifetimes. Were launch to occur at dusk, for example, the resonance would initially be of the first type, resulting in lifetimes several years less (14).

In particular, we show lifetime contours for dipoles which maintain an  $A_{\text{eff}}/M$  value of 50 cm<sup>2</sup>/g. The numbers accompanying the initial orbits lying inside the contours give the corresponding lifetimes in years; outside the outer lifetime contour, the initial orbit conditions are effectively nonresonant. In determining lifetimes for these latter conditions, air density becomes increasingly important; in fact, lifetimes considerably greater than those explicitly indicated in Fig. 4 can sometimes result for initial orbits lying just outside these contours. The numbers accompanying these latter orbits give the minimum perigee altitudes encountered during the first 8 years after launch.

For launch at the time of the winter solstice, the orbit of the planned test belt will lie well within the estimated 8-year lifetime contour. The predicted lifetime, which approximates the average to be expected for dipoles in the test belt, is seen to be about 7 years. For a dipole which maintains an  $A_{\text{eff}}/M$

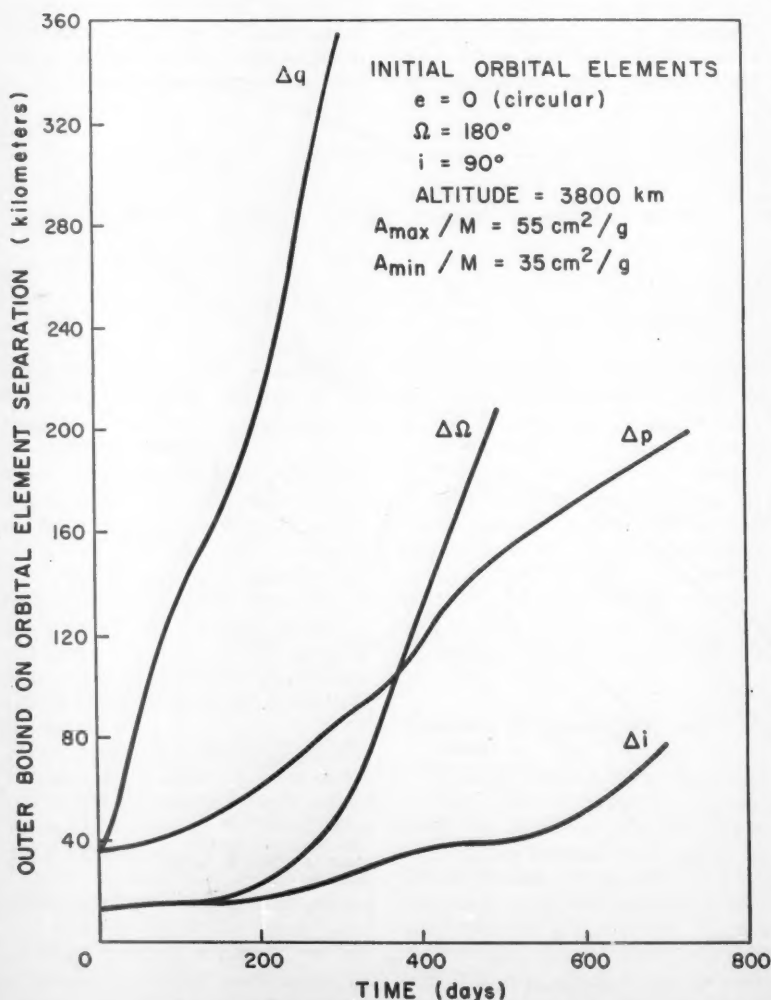


Fig. 5. Outer bounds on separations of dipole orbits.

value of about  $35 \text{ cm}^2/\text{g}$ , we obtain the maximum lifetime. Since, within the resonance region, dipole lifetime is inversely proportional to the average value of  $A_{\text{eff}}/M$ , a maximum lifetime of about 10 years can be expected (15). We emphasize, however, that statistically it is overwhelmingly unlikely for a dipole to spend more than a small fraction of its life with the necessary orientation to yield a value of  $A_{\text{eff}} = A_{\text{min}}$ . Hence only a completely negligible number of dipoles could be expected to have lifetimes corresponding to  $A_{\text{eff}}/M$  remaining at  $35 \text{ cm}^2/\text{g}$  over a period of years.

From our earlier discussion on prediction, we can infer that the exact positions of the contours shown in Fig. 4 are not to be taken too literally (16). Lifetimes for dipoles whose orbits are near the center of the resonance region are probably quite reliable; but for orbits near the border of this region the reliability of predictions is markedly reduced.

### Experimental Lifetime

Communication and propagation studies can be made only while the West Ford belt remains relatively undispersed. To estimate a useful experimental lifetime, we calculate an outer bound on the cross-sectional expansion of the belt.

The action of most disturbing forces is to perturb the orbits of different dipoles differently. These perturbations will thus cause the belt to spread (disperse) far beyond the original size determined by the small differential velocities imparted to the dipoles during dispensing (2).

The major dispersive influence on the polar test belt is that of radiation pressure. Since the dipoles tumble randomly, their effective projected areas vary from  $A_{\text{max}}$  to  $A_{\text{min}}$ , resulting in a corresponding distribution of dipole orbits. Hence, we have used our computer program to calculate the time variations of the orbital elements for the two cases  $A_{\text{eff}} = A_{\text{max}}$  and  $A_{\text{eff}} = A_{\text{min}}$ , the differences giving an outer bound on the expansion of the belt. To obtain rigorous outer bounds we consider dipoles

placed in a resonant polar orbit of the first type; the results are shown in Fig. 5, where the bounds on the separations in the various orbital parameters are plotted as functions of time (17). (These separations are not sensitive to small changes in the initial orbit.) As can be seen, the largest difference occurs in the perigee height ( $\Delta q$ ). During the early stages of the belt this difference grows approximately linearly with time. The curve labeled  $\Delta \Omega$  is an upper bound on the lateral width attained by the belt over the equator, while curve  $\Delta i$  is a similar measure of the belt width over the poles. Curve  $\Delta p$  represents an upper bound on the in-plane separation of the dipoles at the semi-latus rectum points of the orbit. While at perigee and apogee the in-plane spreading is rather large, we see that the separation at the semi-latus rectum points is much smaller, comparable to the out-of-plane separations of the dipoles (18).

Considering the original formation of the belt, we can show that its cross section will be everywhere more or less elliptical. The curves in Fig. 5, then, represent outer bounds for the diameters through these ellipses in various directions and at different points along the orbit. (For example,  $\Delta i$  is an upper bound to the lateral diameter of the elliptical cross section over the poles.)

Since near the semi-latus rectum points of the orbit the belt will remain relatively undispersed, this portion can be used the longest for experimental purposes. Scattering cross-section studies indicate a useful lifetime for communications of about 2 years (19). Near apogee and perigee the expansion is much more rapid, and effective use can be made in these regions only for about 8 months.

### Conclusions

It is expected that sunlight pressure will limit the average orbital lifetime of the Project West Ford dipoles to about 7 years, the useful experimental lifetime of the belt being about 2 years. These conclusions depend on the plan to launch the dipoles into a resonant polar orbit. For such an orbit, un-

certainities in air density have little effect on lifetime predictions whose reliability is supported by comparison with the observed orbit of Echo I (20).

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6. The magnitude of the component of sunlight pressure in the orbit plane varies as the seasons progress. Therefore, even when changing monotonically perigee height will change with a varying rate.
7. We present a more complete discussion of this figure in a later section.
8. For the purposes of this discussion, the changes in  $a$  and  $i$  can be neglected.
9. M. C. Crocker, II, private communication.
10. In the context of this paper, the term *angular momentum* refers only to the motion of a dipole with respect to its center of mass.
11. Some collisions with micrometeoroids will result in the severing of dipoles. Eventually they will be broken into small segments, perhaps of comparable size to the micrometeoroids which are present in the vicinity of the earth in considerably greater numbers.
12. See, for example, F. R. Moulton, *An Introduction to Celestial Mechanics* (Macmillan, New York, 1914), p. 404.
13. Note that since upper atmosphere densities are strongly dependent on solar activity, they will probably decrease in the next few years during the "quiet sun" period.
14. Because of the complicated interplay between the perturbations and the elements, the resonance will actually change with time from the initial type: In long lifetime cases there will be a continuous drift between extreme conditions which approximate first one and then the other of the two resonance types.
15. Since the resonance region is narrower for lower values of  $A/M$ , our lifetime predictions become less reliable.
16. Note, in addition, that some small perturbations were omitted in the computations for Fig. 4. Spot calculations made in the vicinity of the planned test orbit (and including these perturbations) indicated no large changes in the results.
17. The non-zero value of the ordinate at the time of launch denotes the maximum separation due to the maximum difference in dispensing velocity.
18. The separations of the arguments of perigee ( $\Delta \omega$ ), being less than  $2^\circ$  in 2 years, can be neglected in this discussion.
19. D. C. MacLellan, private communication.
20. This work was performed at the M.I.T. Lincoln Laboratory, which is operated with support from the U.S. Air Force. We thank T. F. Rogers for his very helpful comments on the communications aspects of the West Ford belt.

# Fallout from 1957 and 1958 Nuclear Test Series

New York City data show contributions from short-lived  
nuclides for as long as 14 months after testing.

William R. Collins, Jr., George A. Welford, Robert S. Morse

The unique production of radio-tungsten in Operation Hardtack and the moratorium on testing after the autumn of 1958 have made fallout measurements during the past few years very useful in assessing some of the more perplexing aspects of off-site fallout distribution. Several comprehensive summaries and interpretive articles on ground-level contamination during this period (1-5) and new observations on the atmospheric behavior of nuclear debris have been published (6-8). As a result, many questions pertaining to fallout are now answerable. However, the emphasis historically placed on strontium-90 and cesium-137 and the practical difficulties involved in large-scale surveillance of large numbers of nuclides have resulted in a serious lack of detailed information on many of the shorter-lived contaminants that are dispersed by nuclear testing.

This article deals with some of the causes and effects of high concentrations of shorter-lived fission products in fallout in New York City during 1958 and 1959. Data previously reported (9) are supplemented with data on concentrations of strontium-90, cesium-137, ruthenium-106, cerium-144, strontium-89, zirconium-95, and wolfram-185, measured in monthly fallout collections during 1959. Casual examination shows that the shorter-lived fission products predominated over  $\text{Sr}^{90}$  and  $\text{Cs}^{137}$  from the beginning of 1958 through the middle of 1959. This was due primarily to the heavy rate of testing that prevailed during 1957 and 1958, but further interpretation of the measurements indicates that the conditions under which individual test series were conducted during this period also had an effect. Through anal-

ysis of isotope ratios,  $\text{W}^{185}$  concentrations, and monthly rainfall volumes, it has been established (i) that more fallout arrived in New York City from the Soviet series in October 1958 than from earlier series, and (ii) that the Soviet debris was richer in short-lived nuclides because it was deposited soon after its production.

The New York City measurements also provide a means of investigating external doses delivered to the population from photon-emitting fission products. Theoretical gamma-radiation dose rates and integral doses are computed from the reported amounts of  $\text{Zr}^{95}$ ,  $\text{Ce}^{144}$ ,  $\text{Ru}^{106}$ , and  $\text{Cs}^{137}$  that accumulated on the ground during 1958 and 1959. These calculations show that fallout made substantial contributions to open-field dose rates, and that the shorter-lived nuclides, particularly  $\text{Zr}^{95}$ , produced doses comparable to doses of  $\text{Cs}^{137}$ . Since the New York City observations probably are applicable to other sites in the Northern Hemisphere, a more thorough evaluation of the world-wide effects of the shorter-lived nuclides is indicated.

## Methods

Throughout 1959, replicate monthly fallout samples were taken on the roof of the Atomic Energy Commission's Health and Safety Laboratory in New York City with funnel-shaped-ion-exchange collectors (9, 10). At the end of each exposure the paper pulp and resin were removed, ashed at 450°C, and separated into aliquots for determination of gross beta activity. Tungsten, cesium, strontium, cerium, and zirconium fractions were then sequentially separated, purified, and counted

for beta radiation. Disintegration rates were calculated by correcting the observed counts for counter efficiency and background, recovery, self-absorption, build-up, and decay (11). The counting factors for the separated activities were obtained by counting the beta radiation of known quantities of the individual nuclides under the counting conditions for the sample. Self-absorption and efficiency factors for the mixed beta activities were approximated by using potassium chloride as a secondary standard.

When sufficiently high levels of activity were indicated by the initial gross beta-radiation assay, at least two samples from the month's collections were analyzed for  $\text{Ru}^{106}$  by gamma spectrometry. The determination was based on the intensity of the  $\text{Rh}^{106}$  emission peak at 0.51 Mev. The detection efficiency of the scintillator was calculated from the combined  $\text{Zr}^{95}$ - $\text{Nb}^{95}$  peak at 0.76 Mev, with the radiochemically determined  $\text{Zr}^{95}$  concentration as a standard. Niobium-95 was assumed to be in transient equilibrium with  $\text{Zr}^{95}$ , at a daughter-to-parent activity ratio of 2.4. The correction factor was then related to the rhodium measurement through the data of Heath (12) on peak-to-total-emission ratios and total absolute crystal efficiencies. All final determinations were made after the samples had been stored for at least 120 days to minimize interference from  $\text{Ru}^{106}$  emissions at 0.49 Mev.

The ruthenium estimates were confirmed by beta-absorption analyses in which the 3.53-Mev  $\text{Rh}^{106}$  beta component was resolved from the total counting rate of the sample and corrected for counter efficiency (13). In addition, periodic beta-decay measurements were made, from immediately after the sampling period through the end of 1959. Mixed longer-lived nuclides were identified by the decay slope apparent from the later counts and extrapolated back to the original counting date (14). Approximate  $\text{Ru}^{106}$  levels were then obtained by subtracting the measured concentrations of  $\text{Ce}^{144}$ ,  $\text{Cs}^{137}$ , and  $\text{Sr}^{90}$ .

Gamma-radiation doses delivered to a point in air 3 feet above the ground were derived by adapting the method of Hallden and Harley for mixed fission products (15) to specific

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Table 1. Activity levels measured in New York City during 1959. Isotopic activities are reported as of the end of the sampling month. Total beta activity estimates are reported as of the counting date (in parentheses).

Sampling month	Activity levels (mc/mi <sup>2</sup> )							
	Sr <sup>90</sup>	Cs <sup>137</sup>	Ru <sup>106</sup>	Ce <sup>144</sup>	Zr <sup>95</sup>	Sr <sup>89</sup>	W <sup>185</sup>	Gross activity
Jan.	1.00	1.60		30.9	81.7	36.3	22.4	241 (6 Feb. 59)
Feb.	1.81	3.36		49.0	88.6	31.3	16.9	229 (23 Mar. 59)
Mar.	4.45	6.58	48	86.0	90.0	61.1	18.4	356 (29 Apr. 59)
Apr.	4.16	6.93	56	83.1	79.4	39.7	9.84	413 (5 May 59)
May	1.52	1.90	13	19.1	11.3	8.03	4.69	75.7 (9 June 59)
June	2.43	3.46		31.3	15.7	8.01	3.05	97.8 (13 July 59)
July	0.52	0.88		5.69	2.56	<0.78	0.94	21.0 (10 Aug. 59)
Aug.	0.65	0.88		7.24	2.23	<0.49	<0.50	23.3 (8 Sept. 59)
Sept.	0.11	0.20		1.19	1.01	<0.21	<0.20	4.06 (12 Oct. 59)
Oct.	0.39	0.61		2.70	<0.33	<0.36	<0.14	8.30 (24 Nov. 59)
Nov.	0.34	0.51		1.64	<0.24	<0.17	<0.12	5.96 (15 Dec. 59)
Dec.	0.52	0.43		2.53	<0.17	<0.16	<0.068	7.54 (13 Jan. 60)
Total deposition	17.9	27.3		320	419	221	83.8	1480
Activity level on 31 Dec. 1959	36.1	48.5		275	28.1	5.63	9.18	542

nuclides. Emission energies, decay constants, and other pertinent decay-scheme characteristics were taken from the listings of Strominger, Hollander, and Seaborg (16). The radiation sources are assumed to be evenly distributed over an infinite plane, and no corrections are made for weathering, shielding, Compton scattering, or the effects of the ground-to-air interface.

The beta-radiation counters used were standard end-window Geiger-Müller tubes surrounded by anti-coincidence rings and shielded with mercury. The gamma-radiation instrument was a 3- by 2-inch sodium iodide scintillator equipped with a transistorized single-channel analyzer (17).

Monthly fallout activity levels measured during 1959 are listed in Table 1. Each value is the mean of at least two determinations. The Zr<sup>95</sup>, Sr<sup>90</sup>, and W<sup>185</sup> concentrations were considered undetectable after August, June, and July, respectively, when the counting error, expressed as one standard deviation, exceeded the apparent counting rate. In other cases, the relative percentage of uncertainty due to counting factors averaged 3 percent and ranged from 2.1 to 12 percent of the activities reported. The total depositions were obtained by summing the monthly levels and correcting for decay during the sampling period (9). The activity levels shown to exist at the end of 1959 include totals previously reported for the end of 1958, corrected for decay through the end of 1959. The cumulative gross activity estimate for 1958 was obtained by assuming a mean production date of 30 June 1958 and correcting for decay by the T<sup>-1.2</sup> law

(18). The decay corrections for the 1959 monthly increments are the results of actual measurements described in the ruthenium analyses.

Since the counters used were not sensitive enough to detect the less energetic beta emitters, the gross activity estimates are probably low, and more representative of activities with energies in excess of 0.3 Mev. However, these levels do show that at least 5.1 (9) and 1.5 curies, respectively, of beta activity per square mile were deposited in New York City during 1958 and 1959. The predominance of shorter-lived nuclides noted in 1958 continued through the first half of 1959, with the result that the effects of radioactive decay reduced the two years' total deposition by a factor of 10 by the end of 1959.

#### Age and Origin of Debris

It is generally conceded that delayed fallout is primarily of stratospheric origin and that the mean atmospheric residence time of weapon-test debris is from one to three years, depending on testing conditions (5, 6). It follows that the possible major sources of the New York City fallout reported in Table 1 are limited to Soviet, United Kingdom, and United States test series conducted during 1957 and 1958. Although the sensitivity of debris dating methods is lessened by consideration of monthly collections rather than individual rainfalls, the production of W<sup>185</sup> in the United States Hardtack series and the cessation of testing after the Soviet series of October 1958 make

it possible to identify contributions to total fallout from these sources with some degree of accuracy.

From estimated radiotungsten yields for the Hardtack series (6, 8) and measured fission-product yields (atoms per fission) for thermonuclear weapons in general (19), the Sr<sup>90</sup>/W<sup>185</sup> and Sr<sup>90</sup>/W<sup>185</sup> ratios at an assumed mean production date of 1 June 1958 are estimated to be 0.00380 and 0.436, respectively. Monthly depositions of Sr<sup>90</sup> in New York City from Hardtack are calculated directly from measured W<sup>185</sup> levels by extrapolation of the Sr<sup>90</sup>/W<sup>185</sup> production ratio to the reporting dates for the fallout measurements. Levels of Sr<sup>90</sup> from other sources are obtained by subtracting the Hardtack Sr<sup>90</sup> and similarly determined Sr<sup>90</sup> fractions from the total isotopic concentrations and analyzing the Sr<sup>90</sup>/Sr<sup>89</sup> ratios calculated for the non-Hardtack debris.

The interpretation of these ratios is illustrated in Fig. 1. The clearest indication of debris age occurs in the 1959 ratios, which uniformly show the Soviet series of October 1958 to be the predominant source. A second trend indicates contributions from late 1957 and early 1958 testing in samples taken before June 1958. The increases in the ratio observed during the summer months of 1958 are attributed to the arrival in New York City of mixed debris. The compositions of these mixtures are determined by extrapolating the ratio curve for the earlier measurements through the middle of 1959 and

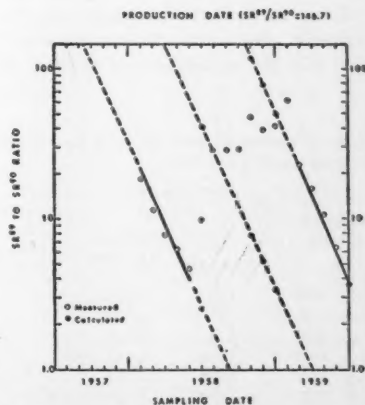


Fig. 1. Fallout from non-Hardtack sources in New York City during 1958 and 1959; Sr<sup>90</sup>/Sr<sup>89</sup> ratios, corrected for calculated Hardtack concentrations, are used. Most of the 1958 data are taken from the report of Welford and Collins (9). Strontium-89 values for the first three months of 1958 are taken from the data summaries of Hardy *et al.* (2).

the 1959 curve back to October 1958. Points falling in between are then resolved algebraically into two values (black dots in Fig. 1) that coincide with the extended curve.

Calculated contributions to total  $\text{Sr}^{90}$  fallout from Soviet testing in the fall of 1958, from Operation Hardtack, and from earlier series are listed in Table 2. Since the  $W^{103}$  levels fell below the detection limits of the collection system used (10), it is not possible to trace Hardtack activities in samples taken after July 1959. An additional difficulty for this period is the possibility that there was fallout from U.S. high-altitude devices detonated during the summer of 1958; this has been inferred by Gustafson (1) and others from the detection of  $\text{Rh}^{106}$  in air and ground measurements. However, it is clear that, prior to July, rates of fallout from the Soviet series were as much as ten times as high as rates of fallout from Hardtack, and that the Soviet series delivered a total of at least four times more  $\text{Sr}^{90}$  to the New York City area.

#### Air Activity Concentrations

The rough proportionality that exists between fallout level and amount of rainfall permits the use of the specific activity of rainfall as a relative index of concentrations of activity in air. Thus, with knowledge of the sources of debris and monthly precipitation totals, it is theoretically possible to follow the fluctuations in air levels that resulted in the  $\text{Sr}^{90}$  depositions listed in Table 2. With the additional assumption that the atmospheric dispersion of

Table 3. Integrated gamma-radiation doses delivered by principal gamma-emitting pairs in fallout in New York City during 1958 and 1959. The 30-year dose from the estimated total  $\text{Ru}^{106}$ - $\text{Rh}^{106}$  deposition during 1958 is given in parentheses.

Sampling quarter	30-year gamma dose (millirads)			
	$\text{Cs}^{137}$ - $\text{Ba}^{137}$	$\text{Zr}^{95}$ - $\text{Nb}^{95}$	$\text{Ru}^{106}$ - $\text{Rh}^{106}$	$\text{Ce}^{144}$ - $\text{Pr}^{144}$
Jan.-Mar. 1958	3.21	4.10		0.105
Apr.-June 1958	4.36	4.42		0.205
Jul.-Sept. 1958	2.93	2.08	(1.17)	0.0862
Oct.-Dec. 1958	3.23	7.39		0.166
Jan.-Mar. 1959	7.20	6.24	1.20	0.284
Apr.-June 1959	7.67	2.05	1.24	0.217
July-Sept. 1959	1.22	0.0925	0.178	0.0230
Oct.-Dec. 1959	0.967	0.0	0.125	0.0115
Total 30-year dose	30.8	26.4	3.91	1.10
Total 70-year dose	47.6	26.4	3.91	1.10
Total infinity dose	56.7	26.4	3.91	1.10

other fission products is not radically different from that of  $\text{Sr}^{90}$ , the build-up and depletion of concentrations of total Hardtack and Soviet debris in the air over New York City during 1958 and 1959 may be traced. Moreover, it is not unreasonable to expect material from possible future polar and equatorial detonations, carried out under the same conditions, to arrive in similar fashion.

Figure 2 compares the fractions of total fission-product yields from Hardtack and total yields from the Soviet October 1958 series found in unit monthly rainfall volumes in New York City after May 1958. The values are derived from the  $\text{Sr}^{90}$  data in Table 2; an average  $\text{Sr}^{90}$  yield of 4.0 atoms per 100 fissions for the two series was used (19). Total TNT megaton equivalents for the two events are taken from the reports of Libby (8) and Martell (6). A mean production date of 18 October 1958 for the Soviet series is calculated from the  $\text{Sr}^{90}/\text{Sr}^{90}$  ratios illustrated in Fig. 1. Rainfall volumes are taken from monthly weather summaries distributed by the United States Department of Commerce (20).

The curves show that both series produced significant levels of specific activity and, by inference, significant concentrations of activity in air in New York City as early as 30 days and as late as a year after their mean detonation dates. Although it is not possible to follow the long-term effects, it appears that the major contributions from the Soviet series occurred over a span of about 150 days, while the dissipation of concentrations from Hardtack required about 250 days. In addition, the Soviet activity build-up was higher by roughly a factor of 10 and occurred about 100 days sooner relative to the date of production than did the Hardtack build-up. The break in the Hard-

tack curve suggests a decline in tropospheric activities after 90 days, followed by the delayed arrival of stratospheric debris after 150 days. As a result of the difference in timing, there was appreciably more shorter-lived material in the fission fragments deposited from the Soviet series.

#### Gamma Dose

From theoretical gamma- to beta-fission-product ratios (15), the total deposition of fallout in New York City during 1958 and 1959 is estimated to have produced from one-half to one-third as many gamma photons as beta particles. Calculated contributions of cumulative levels of representative photon-emitting pairs to total infinite-plane gamma dose rates from fallout are illustrated in Fig. 3. All values are corrected for monthly decay. The  $\text{Ru}^{106}$  levels for January and February and for June through December 1959 were obtained from the  $\text{Ce}^{144}$  measurements by extrapolating the decrease observed in the  $\text{Ce}^{144}/\text{Ru}^{106}$  ratio during March, April, and May. The estimate is believed to be fairly consistent with actual ruthenium levels, since the slope of the decrease agrees with that theoretically calculated from the decay constants of the two nuclides. The estimate for gross 1958 ruthenium deposition is obtained by assuming a  $\text{Ru}^{106}/\text{Ce}^{144}$  activity production ratio of 0.71 (19) and a mean production date of 30 June 1958, as in the approximation for total beta activity.

Although the dose rates shown for the shorter-lived photon-emitting systems are sustained through only a few half-lives of the parent activity, significant integral doses are produced. Table 3 lists the genetically important 30-year gamma doses calculated for

Table 2. Sources of  $\text{Sr}^{90}$  fallout in New York City during 1958 and 1959.

Sampling month	$\text{Sr}^{90}$ activity level (mc/mi <sup>2</sup> )		
	Oct. 1958-Apr. 1959	Operation Hardtack	Soviet, Oct. 1958
May 1958	3.44	0.006	
June 1958	1.23	0.039	
July 1958	1.25	0.224	
Aug. 1958	0.358	0.195	
Sept. 1958	0.378	0.197	
Oct. 1958	0.596	0.227	0.347
Nov. 1958	0.450	0.320	0.420
Dec. 1958	0.119	0.179	0.474
Jan. 1959		0.521	0.479
Feb. 1959		0.512	1.30
Mar. 1959		0.707	3.74
Apr. 1959		0.492	3.67
May 1959		0.313	1.21
June 1959		0.260	2.17
July 1959		0.104	0.416

quarterly isotopic depositions during 1958 and 1959. These contributions are summed to give the total dose that would be delivered if this debris were undisturbed until 1989. These levels are in turn compared with lifetime or 70-year doses and infinity doses.

In relation to ionization-chamber measurements of dose rates from cosmic and natural terrestrial sources made by Solon and his associates (21), the infinite-plane dose rates from fallout in New York City reached as high as 50 percent of total background rates. Suggested correction factors for absorption produced by vertical displacement of fallout activity, Compton scattering of gamma radiation, and partial shielding by topological and architectural structures range from one-fifth to one-tenth of the uncorrected calculated doses (3, 5). Even with the maximum correction, however, dose rates for  $Zr^{95}$ - $Nb^{95}$  fallout alone were between 0.2 and 0.4 microrad per hour for most of the 2-year observation period. Moreover, both the instantaneous and the long-term doses produced by the shorter-lived nuclides were of at least the same order of magnitude as those produced by the long-lived  $Cs^{137}$ - $Ba^{137}$  chain.

## Discussion

As is well known, the use of measured nuclide ratios to study meteorological factors controlling fallout is subject to serious error. In addition, the effects of rapidly changing factors, such as the air mass trajectories and the  $Ba^{140}$  concentrations which Martell (6) has studied, are largely obscured in monthly sampling. Nevertheless, the calculations for Hardtack and for Soviet contributions to total fallout in New York City are in fair agreement with Martell's observations on New England rains, with Gustafson's  $Cs^{137}$  analyses in Chicago (1), and with Lockhart's earlier measurements of  $Sr^{90}$  concentrations in air over Washington, D.C. (7). The extension of isotopic data to total fallout is admittedly limited by such unknown factors as fractionation of volatile fission products in the fission process, resuspension of deposited debris, variation of actual yields from theoretical values, and the assignment of single apparent production dates to series of tests. These difficulties are illustrated by the anomalous  $Sr^{90}/Sr^{90}$  ratios shown for the August and September 1958 samples in Fig. 1 and the uniform

divergence of  $Ce^{144}/Sr^{90}$  and  $Zr^{95}/Sr^{90}$  ratio curves calculated for 1959 from those obtained from theoretical decay constants. Precise knowledge undoubtedly awaits more exhaustive study, but that there is more sustained and more diffuse fallout from the equatorial tests as compared with the polar series appears to be established.

Generalization of these findings to future testing is limited because the peak depositions of Soviet debris are attributable to seasonal effects as well as to latitudinal considerations. Absolute assay of either factor could only be achieved if one factor were held constant—that is, if Hardtack and the Soviet series had both occurred near the poles but at different times, or if they had been conducted simultaneously at different latitudes.

The significance of the data in terms of radiation dose levels is more clearly discernible. The calculated contributions of the fallout that accumulated on the ground in New York City during 1958 and 1959 to external-gamma-radiation dose rates and to long-term doses are illustrated in Fig. 3 and Table 3, respectively. The values are in agreement with the results of gamma-radiation spectrometric analyses of soils

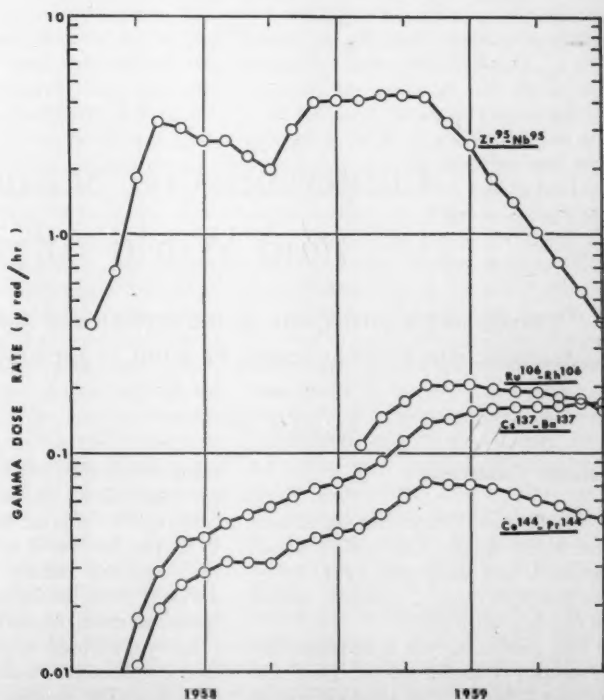
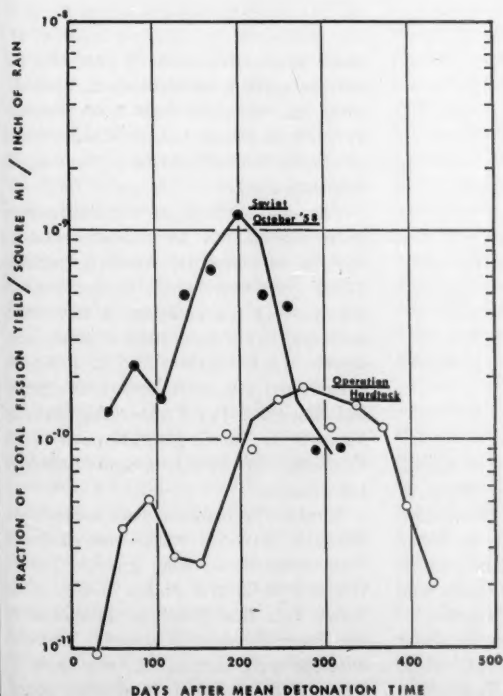


Fig. 2 (left). Comparison of the arrival patterns in New York City of debris from polar (Soviet) and from equatorial (Hardtack) test series conducted during 1958. Fig. 3 (right). Calculated infinite-plane dose rates from cumulative levels of gamma activities measured in fallout in New York City during 1958 and 1959.



from similar latitudes in both the United States (1, 4) and the United Kingdom (3). The soil levels of  $Zr^{90}$ - $Nb^{90}$ , which are less affected by the 1957 and earlier fallout than are levels of the longer-lived nuclides, show a maximum variation of 20 percent from the New York City cumulative fallout levels. In addition, the results of systematic ionization-chamber measurements of open-field dose rates performed by Vennart (3) in Belmont, Surrey, England, closely parallel the  $Zr^{90}$ - $Nb^{90}$  dose rate curve of Fig. 3. These observations imply (i) that weathering and roughness of terrain had little effect on doses from fallout activities during the two-year period, and (ii) that the most significant contribution to total-radiation dose rates was made by radioactive zirconium, or possibly by other fission products of comparably short half-life.

Although the beta emitters in fallout are not sufficiently energetic to cause significant external doses except through direct deposition on body surfaces, internal doses do occur through inhalation and ingestion of debris particles (5). In addition, the chemical similarity of some of the radionuclides to elements normally assimilated by the

body results in concentrated doses to specific tissues. The over-all effect of radiation on the population cannot be known, therefore, until many complex meteorological, physical, chemical, and biological factors are thoroughly understood. It is clear, however, that any valid assessment of the effect of radiation, past or future, must include consideration of fallout and more realistic treatment of the short-lived fission products.

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## Conferences on Science and World Affairs

Statements by participants at the seventh and eighth conferences, which met at Stowe, Vermont, in September.

### Seventh Conference

The Seventh Conference on Science and World Affairs was held at Stowe, Vermont, 5-9 September 1961. Forty-one scientists from 12 countries attended (see box, page 987).

This conference had as its theme "International Cooperation in Pure and Applied Science." Our previous conferences have been chiefly concerned with ways of preventing the misuse of science in the wholesale destruction of

mankind. In this conference at Stowe, we have turned to the discussion of constructive international cooperation in science, because it is a way to create trust between nations, a trust which develops from common interests and from experience in working together.

Science misused by nations to foster their competitive interests as world powers makes possible the destruction of mankind. Science used cooperatively by all nations for the increase of human knowledge and the improvement of

man's productive capacity can give all men on earth a satisfactory and worthwhile life. Scientists bear a responsibility both to foster the constructive use of science and to help in preventing its destructive use.

The deliberations of the conference were carried out in plenary sessions and in meetings of working groups. These groups were six in number, as follows: (i) Cooperation in the Earth Sciences; (ii) Cooperation in Space Research; (iii) Cooperation in the Life Sciences; (iv) Cooperation in the Physical Sciences; (v) Cooperation in Assistance to Developing Nations; (vi) Exchange of Scientists and Scientific Information.

Similar suggestions for cooperative research activities arose independently from different working groups. This is reflected in several places in this statement. This is a welcome indication of the essential unity in science. The discussions were carried on in a spirit of friendly cooperation, and full agreement was reached by the entire conference on the suggestions that will be enumerated in the following paragraphs.



## Cooperation in the Earth Sciences

The planet earth is the common abode of all humans. They have a common interest, both intellectual and practical, in increasing the knowledge of the structure and dynamics of the earth.

As the work of the IGY has demonstrated, the earth sciences present an especially appropriate and fruitful field in international collaboration. The work in these fields must be carried forward on an international scale, as no one country is likely to provide sufficient funds to conduct on its own the world-wide investigations which are required.

In recognition of the developing sociological and engineering problems posed by the present trends in earth sciences, we recommend the enlisting of the social and engineering sciences in this work.

The following concrete proposals are made.

### *A survey of the entire ocean in three dimensions.*

1) *The ocean floor.* We propose an international program to develop a detailed map of the floor of the world ocean, including sub-bottom reflecting layers. This program would require, in the first instance, international agreements on exchange of data and methods, including intercalibration of instruments.

2) *Waters of the ocean.* An international program to survey and map the three-dimensional distribution of temperatures, salinity, density, dissolved oxygen, and nutrient salts, under average conditions, of the ocean, and synoptic surveys to develop the broad picture of seasonal and short-period changes in more limited areas, as well as the study of the interactions among the major bodies of water in the ocean.

3) *Ocean life.* An international survey and mapping showing the major biological provinces of the ocean, and determination of the fertility of the waters at all levels in the food chain and the standing crop of food materials available for human use.

Prosecution of the foregoing program would necessarily involve the establishment of world-wide navigation and communication systems and allocation of radio frequencies for earth-science measurements, which would serve many other significant objectives. These matters, as well as the new ocean-wide surveys recommended above, fall within the province of the International Oceanographic Commission (IOC).

## *Earth's crust and mantle.*

1) *Deep drilling program.* The objective of drilling through the earth's crust to the mantle at selected points around the globe presents many unsolved technical problems which call for international collaboration. We agree, therefore, that the calling of an international conference on these problems is an urgent first step. The conference would provide for the exchange and pooling of drilling techniques developed by present national programs and would consider methods for the solution of such unsolved problems as the re-entry and management of high temperatures to be encountered in depth even at oceanic sites. The survey and selection of drilling sites would be a primary responsibility of this conference. The conference could also undertake the establishment of scientific objectives for the drilling program. We further agree that the execution of the program would benefit from the continuous exchange of technique and personnel, and that the full exchange of information and of samples is imperative for the success of the program. It is recommended that when the prime contractor for the United States Mohole Project is selected, arrangements should be made for such meetings and the exchange of accumulated experiences.

*Total environmental forecasting.* The water-air interface appears to be the primary site of heat, water-vapor, and momentum exchange between the oceans and the atmosphere, and so the key to short-term and long-term forecasting of weather, climate, and changes in the ocean. Together, the oceans and atmosphere constitute a huge and complex heat engine. To some degree the ocean-half of this system acts as the flywheel over both short and long time periods. Studies of these phenomena must necessarily be conducted on a long-term and world-wide basis.

An international conference should be called to consider and to organize the establishment of a world-wide network of radio-telemetering observational buoys. This system of buoys would render continuous reports on atmospheric conditions and so contribute to the completion of the world weather map. It would also continuously monitor the energy and water-vapor energy exchange between the ocean and the atmosphere and would observe the changes in the flow of ocean currents in three dimensions. Over a sufficiently long time period, such a network of buoys would help to assign reliable

values to the rate of overturn of the ocean as a whole, a key problem in both climatological forecasts and the safe disposal of radioactive wastes at sea. The hydrographic offices of the major maritime nations, as well as the World Meteorological Organization (WMO) should be encouraged to stimulate such a program.

## *Resources.*

1) *Fresh water.* The rising world population and the increasing concentration of that population in metropolitan centers is already pressing upon the water resources easily available for direct human consumption, for industrial purposes, and for agriculture. We agree that an international conference should be called to consider the organization of an International Hydrologic Decade for the study of the many unknowns that surround this ominous development.

The proposed conference would institute a world-wide survey of water resources and of the future course of water use. Such a program would have the significant incidental benefit of attracting the interests of scientists to a field in which basic studies have been neglected. Subjects for immediate consideration are the qualities of water, considered from the point of view of various uses; the economical desalting treatment of water; the recycling and the reuse of water. The conference necessary to set such studies in motion should be called by UNESCO.

2) *Living resources of the ocean.* Acre per acre, the oceans today sustain at least as large a plant crop, on the average, as does the land, yet man now gets only about one percent of his food requirements from the sea. The oceans, therefore, offer a means for the rapid solution of the protein deficiency afflicting two-thirds of the world population. It is apparent that these resources can best be exploited to this end through international cooperation. The ultimate aim should be to elevate the fishing industry from a hunting industry to an agricultural technology. It has been estimated that existing fishery techniques could easily produce five times the present annual crop of fish, which is at present about 30 million metric tons. This yield could be even more greatly increased by development of means for helping the winds to overturn the oceans more rapidly. This is not an insurmountable undertaking, but experiments to this end must be conducted under international auspices [the Food and Agricultural Organization (FAO) and the Scientific Commission

for Oceanographic Research (SCOR)].

3) *Mineral resources of the ocean floor.* Recent studies of the ocean floor show a vast reserve of minerals, especially nickel, cobalt, copper, and manganese. By means of bottom-photography organized on a world-wide basis, the potentiality of this reserve may be more fully assessed. The necessary observational program could be organized by SCOR.

4) *Natural catastrophes.* The forecasting, minimizing, or eventual control of violent natural catastrophes is one objective of the earth sciences which will also require continuing international cooperation. Earthquakes, tsunamis, volcanic eruptions, hurricanes, and tornadoes constitute the principal hazards of this kind. With respect to the first three, a better international seismological network, including better instrumentation and distribution of stations, is required. More detailed studies of hurricanes and tornadoes would reveal whether or not man can exert some control over these phenomena.

To implement these programs, we further recommend:

1) That, insofar as possible, existing international organizations such as IOC, UNESCO, FAO, WMO, SCOR, and SCAR (Special Committee on Antarctic Research) be the means of furthering these programs.

2) Since what is suggested here represents a long-term enlargement of their present activities, increased funds may have to be requested to finance continuing international conferences and studies in these various fields. At the outset, at any rate, the funds necessary to finance the travel of specialists to conferences so as to plan productive international programs are relatively small, and within the means of the agencies suggested. If forward-looking programs are aged to, as was the case during IGY, it is likely that each nation will find means of financing its share of the total long-range program.

3) There is at present no effective means of promoting atmospheric research on a world-wide basis. The World Meteorological Organization remains a data-handling organization. At present, to suggest an international atmospheric research center on a scale considerably larger than the international meteorological research institute at Stockholm would tend to rob national centers. Thus it is recommended that this matter, however desirable, be deferred to one of our subsequent conferences. For the present a considerable

acceleration of international atmospheric research seems impractical.

4) Of the many desirable studies suggested the highest priority should be given to three: (i) the world-wide survey of the oceans, including their contents in three dimensions; (ii) the deep drilling program, including systematic samplings of the unconsolidated sediments; (iii) the establishment of a total-environmental—that is to say, atmosphere and ocean—forecasting service.

### Cooperation in Space Research

Though believing that there should be close cooperation in space research, we realize that complete cooperation in this field, as in some others, will become possible only when the arms race is ended, international tensions are reduced, complete and general disarmament becomes a reality, and the need for secrecy disappears.

We believe, however, that certain advances in the presently established cooperation in space are possible now, and that certain others could be studied now with the hope of realization in the not too distant future.

1) First, we recommend an increase in the exchange of scientific information in areas such as the physics of space and the effects of the space environment on life. We further recommend periodic international symposia devoted to such subjects.

2) We also recommend the exchange of methods, and of information on instruments, for scientific space studies which have no military importance.

3) We recommend the orderly assignment and use of radio frequencies in space. We support the initiative in this direction taken by the International Telecommunications Union, and hope that a final solution can be reached within 2 years.

4) We recommend the expansion of existing systems of satellite tracking stations and bilateral agreements between different nations, including (within the limits of military security) an agreement between the United States and the U.S.S.R., on the common use of tracking stations; such a bilateral agreement could serve as the first step in this direction.

5) International agreement should be reached on a coordinated program for the use of rockets and satellites during the Quiet Sun Year (planned for the second half of 1962).

6) Similar agreement should be

reached on cooperation in the use of rockets and satellites in the projected world magnetic survey. This agreement should include arrangements for simultaneous observation from satellites launched into different orbits by different nations. This cooperation could be organized in the framework of the Committee on Space Research (COSPAR).

7) We endorse in principle the desirability of international world-wide systems of communications satellites and of meteorological satellites, since these would clearly be in the interests of all mankind. We realize that certain difficulties now stand in the way, but we hope that the governments of the United States and the U.S.S.R., as well as of other nations embarking on rocket and satellite programs, will undertake a common study of the ways to overcome them.

8) We recommend that cooperation should be established in the instrumental study of the moon, and also that the basic principles of the International Antarctic Treaty be applied to the moon and other cosmic bodies.

9) We recommend the calling of an international conference or symposium to consider how to avoid the biological and radioactive contamination of extra-terrestrial bodies.

### Cooperation in the Life Sciences

Among the many fields of biology in which international cooperation is possible, some are particularly well suited by their nature and importance for combined efforts. These, which relate especially to the promotion of human welfare, have been our primary concern. It is very evident that world-wide betterment of human welfare could be produced if the already existing biological knowledge were to be properly disseminated and fully utilized. Among the most important considerations are the means of bringing this about, so that local facilities and resources can be optimally exploited. The following recommendations incorporate proposals designed to achieve this end, as well as to promote research for the discovery of new knowledge in the field of biology.

*Biological aspects of food resources.* One of the most important problems facing humanity is that of assuring an adequate supply of food. While some areas have an abundant food supply, in others there is a low yield of food production and a correspondingly low level of nutrition. Adequate biological in-

## Scientists Who Participated in the Seventh Conference

Sir John Crawford, Australia  
Hans Thirring, Austria  
C. Pavan, Brazil  
G. Nadjakov, Bulgaria  
G. Burkhardt, Federal Republic of Germany  
Sir Edward Bullard, Great Britain  
A. Haddow, Great Britain  
Sir Ben Lockspeiser, Great Britain  
J. Rotblat, Great Britain  
F. B. Straub, Hungary  
G. Bernardini, Italy  
T. Toyoda, Japan  
B. V. A. Röling, Netherlands

A. A. Blagonravov, Soviet Union  
N. N. Bogolubov, Soviet Union  
M. M. Dubinin, Soviet Union  
V. M. Khvostov, Soviet Union  
N. M. Sissakian, Soviet Union  
N. A. Talensky, Soviet Union  
I. E. Tamm, Soviet Union  
A. V. Topchiev, Soviet Union  
Harrison Brown, United States  
William Consolazio, United States  
Paul Doty, United States  
Bentley Glass, United States  
C. O'D. Iselin, United States  
Martin Kaplan, United States

Chauncey Leake, United States  
Linus Pauling, United States  
Jay Orear, United States  
W. Pickering, United States  
Gerard Piel, United States  
I. Rabi, United States  
Eugene Rabinowitch, United States  
Roger Revelle, United States  
Alexander Rich, United States  
Walter Rosenblith, United States  
Eugene Staley, United States  
Alvin Weinberg, United States  
Eugene Wigner, United States  
J. R. Zacharias, United States

formation now exists to enhance food production in these areas considerably, provided the knowledge is adequately diffused and applied. This can be done most effectively by the development of regional agricultural experiment stations, which can deal with the problem of developing agricultural methods suitable to the local terrain, as well as the long-term problem of breeding plants and animals which are able to thrive in the specific locality. These local agricultural experiment stations should be coordinated with an international center. In this way it will be possible to teach agricultural practices which avoid errors experienced in the development of other areas, such as erosion of the soil, deforestation, and overgrazing. The implementation of this recommendation might be carried out through the establishment under the United Nations of an international center and a large-scale international training program, supplemented by interinstitutional exchanges of personnel between countries. Although some work has been carried out under the United Nations in this area, its program should be strengthened and extended considerably, especially in the newly developing countries.

Even though the majority of all life on this planet is synthesized in the oceans, man has utilized this source of food only to a limited degree. It is quite likely that the oceans can supply ten times more food for man than the total being produced at the present time. This can be done by developing methods for the cultivation of food in the oceans, rather than the use of the inefficient and self-limiting hunting procedures employed today. To do this we must greatly expand our knowledge of marine biology. Fortunately, the international oceanographic research expeditions, such as that now planned for the

exploration of the Indian Ocean, can afford a significant opportunity for increasing our knowledge of marine life. Another important means for expanding knowledge in this area is through the work of marine biological laboratories, situated in many countries. Many of these laboratories are now critically short of funds and need a wider basis of support. We recommend that these laboratories be united into an international system, perhaps under the International Union of Biological Sciences, to ensure permanent support and increase in number.

*Preservation and promotion of health.* The health sciences offer one of the most rewarding meeting grounds for international cooperation in science. Increased international cooperation and financial support is essential for the realization of important advances in such fields as cancer, cardiovascular disease, immunology, infectious diseases, mental health, environmental sanitation, problems of aging, nutrition, human genetics, and others.

International institutes of health devoted to these problems should be established in one carefully chosen place to serve as a world center of medical research. Sufficient financial support of a long-term nature should be provided to permit their efficient operation. Smaller subsidiary institutes oriented towards more specialized problems or regional needs (space medicine, medical entomology, tropical medicine) should be established in different countries.

Other urgent needs in the health sciences characteristically requiring greater international cooperation and support include: (i) more effective and rapid dissemination of information on research and advances in medical knowledge; (ii) increased education and training of physicians and allied scientific

and auxiliary personnel at both undergraduate and postgraduate levels; (iii) an extension of epidemiological studies and control of important communicable diseases, such as malaria, tuberculosis, and influenza, as well as of chronic degenerative diseases; (iv) genetic and immunologic studies of human population groups; and (v) problems of reproduction.

The central coordination and administration of these institutes and programs should be entrusted to the World Health Organization of the United Nations, which is the major operating international agency in the medical field. It is realized, however, that these additional tasks in the health sciences cannot be undertaken adequately unless the funds now being spent for these purposes are at least trebled.

*The environment and its modification by man.* The exponential growth of human populations and the accompanying industrial, agricultural, and scientific activities have given rise to a number of serious problems, including pollution of air and water resources, which are of considerable biological importance. We now recognize that continual chemical pollution of the air (known as smog) is a characteristic feature associated with most metropolitan areas and, as such, constitutes a problem of worldwide importance. The great increase of industrial growth has brought about extensive water pollution, which is lethal to aquatic organisms and renders the water unfit for human use. Especially grave are the problems involving contamination of air, soil, and water with radioactive substances. Included here, as well as the more obvious problem of fallout from nuclear explosions, is the matter of the safe disposal of radioactive wastes. Even though several agencies of the United Nations have carried



out extensive studies in the field of radioactive contamination, more international attention should be directed toward the problems of chemical pollution of air and water. We recommend that international conferences be scheduled on these topics.

Another of the more serious consequences of man's rapid growth is the extent to which it may bring about the extinction of many plant and animal species. There are large forest and game reserves in the newly formed African countries which are seriously threatened today because of a shortage of funds and trained personnel who can maintain these preserves. This problem has been considered by a committee on ecology in the International Council of Scientific Unions. We believe that action on this problem should be taken by the United Nations in order to bring about prompt and effective results. If this is not done rapidly we will unfortunately suffer irreplaceable losses. Another aspect of this problem should be the establishment of a system of world-wide institutes for preserving indigenous strains of plants, animals, and microorganisms. These may have enormous practical as well as theoretical benefits in future years.

*"Endless frontiers."* It has been said that the developments in biology during the next century will be as explosive as the growth of the physical sciences in the preceding century. This will undoubtedly offer new opportunities for international cooperation. At least two directions of this advance are already evident.

There has been a phenomenal development recently in our understanding of the structure and function of biological macromolecules and the central role of the nucleic acids and the mechanism which relates the nucleic acids to the protein molecules. This work has led to significant insight into the molecular events which underlie cell division, as well as viral infection. These developments have significant bearing on the problem of cancer as well as the broader fields of molecular evolution and the origin of life. Intense interest in this field has developed among scientists in all countries, and it may be possible to capitalize on this enthusiasm by developing an intercontinental institute of molecular biology. This institute could serve as a research and training center for expediting the development of the subject and as an important continuing channel of communication in the bio-

logical field between "East" and "West" countries.

This proposal originated in both the biological and physical science working groups.

Both the United States and the Soviet Union have announced that they are planning to carry out manned exploration of space. It is not unreasonable to suggest that some of the biological developmental work be carried out in common. To implement this exploration it will be necessary for these proposed trips to develop a wide variety of equipment needed to maintain the human occupant for a prolonged period in a confined space. A considerable saving of time and money would ensue from joint research projects in this area. In addition, some of the instruments which man uses in space for his scientific investigations could be included in this cooperative program. Joint precautions must be exerted to prevent the contamination of extraterrestrial bodies by terrestrial organisms. Joint investigations of possible extraterrestrial forms of life and macromolecules should be planned. We propose that these suggestions be forwarded to COSPAR for its consideration. It should be noted that the adoption of even a limited type of cooperation in space research would be of great symbolic value and have substantial popular appeal.

At the present time an International Biological Program is under discussion by the International Council of Scientific Unions. Many of the projects described above may be included in this program, which if carried out broadly and effectively, would have considerable scientific value as well as a favorable impact on public opinion.

#### Cooperation in the Physical Sciences

Modern physical science has in many of its aspects become very big and expensive. It therefore lends itself particularly well to intercontinental cooperation in which the costs are shared, and the results are made available to all mankind.

Four specific areas of physical science were identified as being ripe for vigorous action on an intercontinental basis. These areas were the following.

*High-energy physics.* The field of high-energy physics is an excellent one for cooperation between all countries of the world. This cooperation could center around the establishment of a labora-

tory whose main research tool would be an accelerator of not less than  $300 \times 10^9$  electron volts and of a design which would achieve success in the shortest possible time.

*Controlled thermonuclear and plasma research.* In the field of controlled thermonuclear research there has been much effective exchange of information and scientists. This development is particularly significant since thermonuclear research, prior to 1955, was secret. We urge that such collaboration be broadened—in particular, that the world's thermonuclear laboratories remain open to scientists of all nations who can contribute to this interesting and potentially important field of research.

Although a new, very large thermonuclear device is probably not needed immediately, still there is a large field of general research in plasma physics which could well be advanced by the establishment of an intercontinental laboratory.

*Ultra-heavy-element chemistry.* The production and study of the very heavy elements (atomic number 95 and above) and the resulting extension and elucidation of Mendeleev's periodic chart is a scientific investigation of great interest to mankind. Handling large quantities of the heavy elements is difficult and very expensive, and gives strong reason to pool the world's efforts in this field.

Two different devices are needed for such studies: high-flux reactors and heavy-ion, high-current cyclotrons.

We recommend that an intercontinental center devoted to investigation of the properties of the ultra-heavy elements be established. The center probably should be equipped with the most powerful available heavy-ion cyclotron and with equipment for handling the materials. The ultra-high-flux reactor ( $10^{16}$  neutrons per square centimeter per second), because of its hazards, probably should be located at a different, more isolated site.

*Large-scale computers.* The development of the large modern electronic computer, with its enormous memory and high speeds, represents one of the most significant scientific events of the last two decades. The future development of these computing machines, with larger memories and higher speeds by orders of magnitude, would be of immense value to science. Such computing machines will cost sums of the order of magnitude of a large accelerator—perhaps as much as  $\$100 \times 10^9$ .

The development of such computers



would be a suitable project for international cooperation. The utilization of such a machine will advance not only mathematics but all the physical sciences and the biological sciences, particularly the unraveling of the structure of macromolecules. It would also find great utility in economics and other social sciences.

We recommend further study for such an intercontinental center.

*A globular cluster of big science centers.* It is our belief that the separate big science laboratories in high-energy physics, heavy-element chemistry, macromolecular biology, health research, and possibly thermonuclear research will prosper better if they are reasonably close together than if they are completely isolated from each other. We particularly believe that the intercontinental computing center will be more viable and will be a better center if it is the nucleus of such a cluster.

We therefore urge that the Intercontinental Scientific Laboratories be located in relative geographic proximity and that they be served by the Intercontinental Computing Center. Such an Intercontinental Science Center, comprising much of what is called "Big Science," would represent a capital investment of the order of  $\$5 \times 10^9$ . It is our belief that the astute location of such a striking epitome of science—the most characteristic theme of our modern civilization—could have extraordinarily great significance in improving the tone of the present political situation.

#### **Cooperation in Assistance to Developing Nations**

We express the strong belief that assistance to developing countries is a duty and necessity for all countries.

This aid should be rendered so that it would not impair the independence of any country.

We express our support for greater international cooperation in assistance to developing nations. Such cooperation could help to reduce world tensions, to strengthen peace, and to further disarmament. Disarmament would in turn improve the climate for international cooperation in this and other fields and make available additional funds which could and, we hope, would be used to increase the assistance to developing nations. Clearly, the greatest cooperation and the release of maximum funds

for the assistance program could be achieved by complete disarmament.

Assistance, we believe, should be provided on both a bilateral and a multilateral basis, including a substantial expansion of assistance through the United Nations.

The problems of assistance to developing nations are different in different parts of the world and should be studied as such. Scientists could assist in this study through a cooperative program, which should involve not only scientists of different countries but also specialists in different fields—biologists and physicists as well as anthropologists, economists, and engineers.

Several programs have to be undertaken simultaneously if technical assistance is to be successful. These include measures of immediate help as well as programs which require long-range approach. Coordinated study of such programs by scientists from different countries, including both scientists from the developed countries and those from the recipient countries, is required. Useful in this connection may be further spread of "sister" relations such as already exist between some universities in the developed and in the developing nations. Similar relations could be established between research institutes, agricultural stations, and other centers of applied research. Particular attention should be given to problems of adaptation of advanced technology to the needs and resources of an underdeveloped area. Establishment of regional applied science research institutes appears desirable.

We believe that scientists have a definite role to play in the development of assistance programs and therefore suggest that we place special emphasis on this subject in one of our subsequent conferences. We believe that this conference should include a large participation of representatives from the developing nations.

We welcome the recommendation from the Economic and Social Council (ECOSOC) that a conference on science and technology in application to the problems of new nations should be held next summer in Geneva under United Nations sponsorship.

Among subjects related to assistance to developing nations in which scientists have a particular interest are problems of education in all its aspects—science education and general education, elementary education and higher education. Another subject of interest to sci-

entists is that of natural resources of different areas and of their population trends. Unprejudiced quantitative study of the latter topic should be carried out on an international, cooperative basis. Another topic for similar cooperative research is world nutrition, including the study of reasons for widespread occurrence of avoidable malnutrition in many parts of the world. The problems of energy supply, particularly in areas in which the demand for energy is diffused rather than concentrated in large industrial centers, calls for a similar study. We suggest that preliminary study groups on these problems be organized prior to the above-mentioned conference.

We suggest that an international study be made of the advisability and practicability of establishing an international fund to which individual scientists from countries in which local funds are insufficient for this purpose could apply for assistance in their scientific research.

#### **Exchange of Scientists and Scientific Information**

By its very nature and tradition science is a universal enterprise. Not only does the accumulating knowledge and understanding belong to all mankind, but the work of science moves forward most surely when it engages the collaborative effort of scientists of all nations. The intimate character of this collaboration on questions of profound import to the life of mankind serves to enhance the mutual trust and understanding of the scientific community and of the nations from which its members are drawn.

The rapid exchange of information, mutual visits of scientists, and their working as guests in the laboratories of other scientists constitute the main pathways of scientific collaboration among scientists all over the world. We note that there has been substantial progress in recent years in the area of scientific exchange. Scholars of many nations have the opportunity to meet and discuss scientific questions at international conferences, symposia, and other meetings organized by international scientific organizations as well as by the national organizations of scientists and institutions of higher learning. In this epoch of accelerating progress in science, the fostering of such international contacts and exchanges has become an

## Scientists Who Participated in the Eighth Conference

Sir Mark Oliphant, Australia  
G. Nadjakov, Bulgaria  
J. Polanyi, Canada  
G. Burkhardt, Federal Republic of Germany  
Francis Perrin, France  
Pierre Rosenstiehl, France  
P. M. S. Blackett, Great Britain  
Sir Edward Bullard, Great Britain  
Sir John Cockcroft, Great Britain  
Michael Howard, Great Britain  
Rt. Hon. Philip Noel-Baker, Great Britain  
Sir William Penney, Great Britain  
J. Rotblat, Great Britain  
F. B. Straub, Hungary  
T. Toyoda, Japan

B. V. A. Röling, Netherlands  
N. I. Bazanov, Soviet Union  
A. A. Blagonravov, Soviet Union  
N. N. Bogolubov, Soviet Union  
M. M. Dubinin, Soviet Union  
V. M. Khvostov, Soviet Union  
S. G. T. Korneev, Soviet Union  
V. P. Pavlichenko, Soviet Union  
N. M. Sissakian, Soviet Union  
N. A. Talensky, Soviet Union  
I. E. Tamm, Soviet Union  
A. V. Topchiev, Soviet Union  
Hans Bethe, United States  
R. R. Bowie, United States  
Donald Brennan, United States  
Harrison Brown, United States  
Paul Doty, United States

B. T. Feld, United States  
Trevor Gardner, United States  
Bentley Glass, United States  
Amrom Katz, United States  
Henry Kissinger, United States  
George Kistiakowsky, United States  
Charles Lauritsen, United States  
Leon Lipson, United States  
W. Panofsky, United States  
E. M. Purcell, United States  
I. Rabi, United States  
Eugene Rabinowitch, United States  
Matthew Sands, United States  
Louis B. Sohn, United States  
Leo Szilard, United States  
Charles Townes, United States

increasingly urgent necessity. During the past decade, the accomplishments of the International Geophysical Year have provided triumphant demonstration of the fruitfulness of international cooperation in science. Moreover, the development of cooperative research among scientists of many countries and the consequent internationalization of Antarctica has given a concrete demonstration of how such cooperation can set precedents for constructive agreements among nations.

We express regret that there exist a number of difficulties which interfere in major ways with the further broadening of scientific collaboration and exchange.

*Exchange of scientists.* We recommend that the planned exchange of scientific personnel initiated by the Bronk-Nesmeyanov and similar agreements should be considerably increased. The visits should be extended over periods sufficient for the completion of research projects. In addition to planned exchange, the framework of these agreements should allow for and encourage invitations to scientists in the country in which they are to visit, and for the invited scientists to be able to accept such invitations. We find that application of the *quid pro quo* principle to visits under the agreement (that is, the exchange of one solid-state physicist for another solid-state physicist, and so on) has tended to hamper fruitful exchange, and we urge that such regulations be set aside in future agreements.

The role of government bureaucracies in the administration of these agreements should be minimized. To implement this recommendation, we urge

governments to expedite visas and passports for scientists, since past and present failures in this respect have seriously hampered scientific exchange.

We recommend that, in addition to exchange via formal arrangements, the other traditional forms of scientific exchange—personal visits and correspondence among scientists, attendance at international scientific meetings and at meetings of scientific societies of other nations, the framing of common plans for joint and parallel research enterprises, and so on—be encouraged and facilitated.

We recommend that the scientific organizations of various nations should consider ways to facilitate the travel of pre- and postdoctoral fellows across national boundaries to study, for adequate periods of time (1 year or more), at research centers and under teachers where their training and scientific maturation can be best enhanced.

*Exchange of information.* Noting the obstacle to the exchange of scientific information presented by the mounting volume of current publication, which is increasing exponentially and doubling about every decade, we feel that the situation calls for radical measures of rationalization, to be designed and carried out through international agreement and cooperation. Among potentially useful measures are the following: to review and coordinate the character and content of journals published in all countries with a view to reducing the number and variety of journals which a scientist must follow to keep abreast of work in his discipline; to institute standard formats for the presentation of scientific papers; to formulate a standard system for the

annotation of the contents of published papers suitable for coding and manipulation by machines for the storage and retrieval of information; to institute regional depots under international coordination to store complete experimental records and other documentation in support of the brief published papers; to make such material rapidly available to interested scientists; to consolidate the abstracting services now carried on independently in many countries—a measure that could reduce present duplication of effort by a conservatively estimated factor of 3.

We recommend that measures be taken to bring significant work going forward in all countries to the attention of interested scientists. This objective could be served by the publication of international review journals of two types: interdisciplinary review journals written in relatively nontechnical language for the benefit of scientists in different disciplines, and more specialized review journals which would keep scientists working in a given area abreast of the work going on in the same or related areas all over the world.

We urge that all governments open their postal systems to the untrammelled flow of scientific publications whatever their country of origin or destination.

### Eighth Conference

The Eighth Conference on Science and World Affairs was held at Stowe, Vermont, from 11 to 16 September; its general subject was "Disarmament and World Security" (see box above).

It is gratifying that in such troubled times it proved possible for 48 scientists from 11 countries to meet in a friendly atmosphere and to examine together carefully the dangers which face the people of the world.

During the previous week the Seventh Conference, devoted to international cooperation in science, had outlined many important areas where co-operative action would be scientifically productive as well as effective in improving international understanding. In this Eighth Conference, a wide range of topics was discussed in plenary session, in separate working groups, and in private conversation.

The subjects of study which related in one way or another to the problems of attaining stable peace, world security, and general and complete disarmament included: cessation of production of fissile materials for military use and destruction of military nuclear stockpiles; elimination and control of means for weapons delivery; demilitarization of outer space; interdependence of international political settlements and disarmament; nuclear weapons tests; military disengagement, and creation of demilitarized and atom-free zones; international security forces;

methods of settlement of international disputes; rules of peaceful coexistence; organization and control and inspection over disarmament; and conditions for creating trust and confidence among nations.

A variety of individual views was expressed. These were often quite divergent but were explored in a frank manner. The participants found the discussions helpful in clarifying points of view, and common understanding was reached on a number of important issues. We hope this will open important avenues for constructive action.

The participants of the conference are united in the realization of the danger of unleashing a nuclear war, which would cause untold destruction and bring death to innumerable people. We hope that the desire for peace and the revulsion against war, which are shared by all peoples, will make possible a peaceful resolution of the conflicts which have led to the present deterioration of the international situation, and make possible the attainment of complete and universal disarmament and the establishment of stable peace on earth.

In the present crisis we reaffirm our belief in the general principles enun-

ciated in the Vienna Declaration of September 1958.

This meeting kept open a much-needed informal channel of communication among scientists concerned with the future of civilization.

For this reason it is hoped that similar conferences will be convened by the Continuing Committee at suitable intervals in the future. In addition, plans have been made to form continuing unofficial East-West study groups in order to devote more detailed attention to problems of the nature of those considered at the present conference.

The National Academy of Sciences and the American Academy of Arts and Sciences were hosts to this as well as the preceding conference. Both conferences were organized by the United States Organizational Committee under the aegis of the International Continuing Committee of these conferences.

The following did not join in the resolution: R. R. Bowie, Donald Brennan, Amrom Katz, Henry Kissinger, and Leon Lipson.

The following were absent during the discussion of the statement: Sir John Cockcroft, Trevor Gardner, Charles Lauritsen, and I. Rabi.

## Science and the News

### U.S. Disarmament Plan: It Puts Inspection in First, Rather than Third, Stage

History records numerous attempts by hostile nations to reach disarmament. These searches for the "Holy Grail" have been futile, largely because those setting out on the quest have invariably sought to restrict or eliminate the best weapons of the enemy. "One's own weapons never threaten the peace; they are defensive in character," as William R. Frye wrote in the arms control issue of *Daedalus* last fall.

The present chapter in this history opened in the wake of World War II

and the introduction of nuclear weapons. These weapons, their subsequent proliferation, their ease of being hidden plus the development of high-speed delivery systems such as missiles, and the interrelation of these factors have enormously complicated the disarmament situation until now the goal poses not only political but highly difficult technical problems. The latest entry in the chapter was made last week when the United States, following the Soviet nuclear test resumption that has brought the world to "thirty minutes from Armageddon," presented at the United Nations its most detailed and comprehensive proposal to date for

"complete and general disarmament."

To appreciate that proposal, some knowledge of background to disarmament is necessary. Disarmament attempts since World War II have gone through three periods. First, in 1946, the U.S. proposed control of atomic energy. Second, in 1952, a United Nations Disarmament Commission was created and worked on and off for a couple of years. Neither got anywhere.

The third period began in 1955 with general disarmament talks. Since then there have been negotiations with the Soviets on limited disarmament measures, such as the nuclear test ban and reducing the danger of surprise attacks; and in March 1960 on broad disarmament again in a ten-nation (five Communist, five Western) conference under U.N. sponsorship. Within a few months, however, in the aftermath of the U-2 flight and the Paris summit break-up, the conference collapsed, and it has been in recess for 16 months. At this writing, despite the new U.S. proposal and an agreement in principle on disarmament ideals between the Soviet Union and the United



States, the conferees have not agreed on terms for reopening the discussion.

Proposals and counter proposals were put forth during each of these periods. The most detailed of them was made by Premier Khrushchev at the U.N. in 1959. It was quickly dismissed, sometimes with ridicule and satire; but Jerome B. Wiesner, now President Kennedy's science adviser, has described it as "indeed more imaginative than any proposed by responsible leaders of a major nation in the postwar period," and that included the Anglo-French plans of 1954-55.

To be sure, Khrushchev's program had holes in it unacceptable to the West, Wiesner wrote in *Daedalus*. It recognized the need for inspection to verify any disarmament program, but only after extensive disarmament had occurred. It called for an "international control organ" but no supranational security force to maintain the peace after disarmament. The inspection position points up the basic split between the Soviets and the West on disarmament: they want a minimum of inspection, fearing espionage; the U.S. wants a maximum of inspection, fearing surprise attack.

#### Soviet Plan

Khrushchev's was a three-stage program to be carried out in a 4-year time span, with each stage to be verified as having in fact been effected before proceeding to the next. The stages were:

- 1) Reduction of armed forces and the weapons at their disposal. The U.S., the U.S.S.R., and Red China would cut their forces to 1.7 million men, Britain and France to 650,000, and so forth; and the amount of arms in the retained forces would be frozen at present levels.

- 2) Dissolution of all armed forces and overseas bases.

- 3) Destruction of nuclear weapons and missiles and air forces; creation of a force to stop production of chemical and bacteriological weapons and destroy existing stockpiles; prohibition of research and development on weapons; establishment of an international control agency to supervise carrying out this program, the agency to "have free access to all objects of control" but only "upon completion of general and complete disarmament."

Wiesner gives several reasons why this and previous plans never went anywhere. For one, "a serious communication block" which saw proposals "evaluated not in terms of in-

tended meanings but rather in terms of the most threatening alternatives." For another, lack of sincerity on both sides in attempts to reach agreement. Finally, lack of preparation; the Americans "had very inadequate technical preparations to support them in the discussions" plus "a lack of any definite national position" on both general and limited disarmament. And the Russians were no better, and sometimes less well, prepared. "Up to the present (Fall, 1960), there has not been adequate examination of the technical details of any comprehensive system to make possible a really satisfactory evaluation of it," Wiesner concluded.

#### United States Proposal

Since then, presumably, this situation has been corrected by thorough studies. The result is the new U.S. proposal. The plan effectively runs together into the first stage of disarmament all of the important steps. In particular, the first and third stages of the Soviet scheme, greatly expanded, are telescoped into the first stage of the U.S. proposal. The U.S. stages II and III call for a "further reduction" in the disarmament already begun.

Stage I calls for establishing a disarmament organization to verify the program, with a single administrator under a board of commissioners of the major powers and neutrals; reducing the armed forces (U.S. and U.S.S.R. levels at 2.1 million, less for other nations); destroying excess arms and limiting arms production; setting up a commission of experts to reduce and eliminate chemical, bacteriological, and radiological weapons; stopping production of fissionable materials and transferring existing materials to peaceful uses; setting up a commission of experts to find and eliminate nuclear stockpiles; reducing strategic delivery vehicles for nuclear weapons to "agreed levels"; destroying or converting the rest of the vehicles to peaceful uses; discontinuing or limiting the production and testing of weapons to counter the strategic vehicles; prohibiting the orbiting of weapons of mass destruction; setting up a U.N. Peace Force; and so forth.

The "logical place to begin" is with a nuclear test-ban treaty, President Kennedy said. He asked for an agreement to stop fallout-producing tests in the atmosphere "without inspection or controls" (the Soviets quickly rejected it); and warned that the U.S. cannot endanger itself with "another long, un-

inspected ban on testing" of all types.

(For the moment, the U.S. is set against merging the test-ban talks with general disarmament negotiations, fearing the former would be submerged in the existing morass. However, it probably cannot long maintain this position, with the moratorium collapsed and the talks indefinitely recessed.)

The thinking behind the U.S. proposal, if Wiesner's article reflects it, comes from two convictions. First, comprehensive arms control measures will gain acceptance more easily and may require no more inspection of Soviet territory than individual, limited measures. Thus strategic delivery vehicles and nuclear weapons are part of the first stage of the plan. The interrelationship of these two systems allows acceptance of less certainty in verifying the stockpiles of either one.

Second, Wiesner states that "a mutually agreed-upon stable deterrent system"—both powers would have enough missiles with nuclear warheads to be certain they can retaliate successfully—"could provide the basis for comprehensive disarmament because it provides a means of reconciling the Soviet reluctance to permit inspections and the Western fear of clandestine weapons." The arms race in these weapons would stop; disarmament in general could go forward. Neither side would have to trust the other; both are assured adequate forces to strike back.

Soviet reaction to the U.S. plan was not favorable. The Russians insist on bringing the nuclear test-ban deliberations into the general disarmament talks. They want neutrals included in the ten-nation disarmament conference (the U.S. will probably go along). They reject control during the first stage of disarmament. Finally, they insist on "troika" administration, in effect a veto power, over the control agency, over the number and destination of its inspectors, and its other functions.

A U.S. offer at the closing sessions of the Geneva test-ban talks may again be put forth in disarmament talks on the "troika" issue. It would give the board of commissioners power to dismiss the single, neutral administrator of the control agency. It was not acceptable then to the Soviets; chances are it will not be acceptable now.

Unless radical changes are made in the Soviet stand, little real progress can be expected, a high Pentagon official said pessimistically. However, an equally "high" official, this one in the



President's official family, was optimistic. Reaction to the newly accelerated arms race will provide some positive force toward an agreement, he said. But beyond that he was hopeful because, in his unquotable words, you have to be to get anywhere on the problem. Disarmament will come, he felt certain. The question is whether it will come about before or after World War III.—ROBERT TOTH.

*While Howard Margolis is on vacation, his section will be written by guest reporters. Robert Toth, this week's guest, is on the staff of the New York Herald Tribune.*

### **U.N. Specialized Agencies: With Few Exceptions, They Are Unaffected by International Political Storms**

The turmoil that currently afflicts the United Nations' political organs has had few repercussions in the U.N.'s numerous scientific and technical agencies.

In contrast to the pessimism and uncertainty that envelop the General Assembly and the Security Council, an atmosphere of business as usual exists in such specialized agencies as the United Nations Educational, Scientific and Cultural Organization and the Food and Agriculture Organization. While these and many of the other specialized agencies are closely associated with the United Nations, they are organically and to a large extent financially independent.

Membership is on a voluntary basis, open to nations regardless of whether they belong to the U.N. West Germany, for example, is not a U.N. member, but belongs to virtually all U.N.-associated agencies. The Soviet Union and most Eastern Bloc nations have not chosen to join FAO—presumably to shield agricultural deficiencies from Western eyes. However, the U.N. dues of these nations help provide U.N. supplemental funds for the FAO budget, and FAO has been carrying out its work beyond the range of political shock waves.

Of particular significance for the various agencies' immunity from international strife is the fact that their programs rarely touch raw nerves in the East-West conflict. When they do, however, the possibilities for effectiveness become extremely limited.

UNESCO, with a current biennial budget of \$32,514,228 of member

funds, plus over \$23 million in funds provided by the U.N., has given priority to primary education in Latin America, arid land research, and the promotion of cultural understanding between the Orient and the Occident. These programs step on no one's toes and in many respects parallel foreign-aid efforts by both the Soviet Union and the United States. UNESCO sources say they are being carried out free of the turbulence now buffeting the political bodies of the U.N.

The political weather around the International Atomic Energy Agency is in sharp contrast. The agency, founded in 1957 on the basis of President Eisenhower's atoms-for-peace proposal, has experienced slow growth because of delays in reactor development and East-West differences over nuclear inspection. Last week, in the detailed U.S. disarmament plan issued after President Kennedy's U.N. address, it was proposed that the IAEA exercise safeguards over the international transfer of fissionable materials. In addition, as was pointed out in the *New York Times* several days later, the U.S. was close to completing a bilateral agreement with IAEA for inspection of the experimental reactors at Piqua, Ohio, and Argonne National Laboratory, and of the graphite and medical research reactors at the Brookhaven National Laboratory.

The object of the disarmament proposal and the inspection agreement, according to an American spokesman at the U.N., was to elevate IAEA's standing as an agency for implementing nuclear control agreements, and to establish for it a role as an international instrument for inspection. The proposal for an IAEA role in the East-West dispute over implementation of an arms agreement set the agency at once apart from the political placidity common to most of the other U.N. agencies.

### **Vienna Meeting**

At IAEA's general conference in Vienna, Vasily S. Yemelyanov, head of the Soviet Atomic Energy Authority, warned against attempting to extend the scope of the agency. He charged that the United States is attempting to use the agency for political purposes, and opposed any steps designed to turn the agency into an instrument of arms control.

While Yemelyanov apparently was reacting to the prospect of IAEA being thrust into the touchy area of nuclear

inspection, American delegates were optimistically announcing progress in Soviet-American discussions on joint construction of a gigantic nuclear accelerator. The motivation for these discussions was the desire to share knowledge and costs.

As in the cases of UNESCO, FAO, and other agencies, East-West cooperation finds fertile ground outside the boundaries of Cold War interests.—D.S.G.

### **General Electric, with Prospects Dimmed by FCC, Drops Bid for Communication Satellite**

The General Electric Company has formally withdrawn its application for participation in the development of a space satellite communication system. G.E.'s action strengthens the commanding position held in this field by the American Telephone and Telegraph Company, and is certain to intensify congressional misgivings about the FCC's apparent predilection for an ownership arrangement that the Justice Department has charged would give dominance to A.T.&T.

General Electric's bid for participation in the potentially lucrative business of space communications was never warmly received by the FCC. A.T.&T., which got a head start in development of space communication plans, proposed to the FCC last spring that the system be limited to international carriers, that is, firms licensed for overseas communication activities. The proposal was countered by G.E., which sought to have the system opened to equipment manufacturers as well as carriers. In a ruling endorsing the A.T.&T. position, the FCC stated that inclusion of the equipment manufacturers could "result in encumbering the system with complicated and costly corporate relationships, disrupting operational patterns that have been established in the international common carrier industry, and impeding effective regulation of the rates and services of the industry."

General Electric's position, backed by a number of major manufacturing firms, was supported by the Justice Department. The antitrust division argued that with A.T.&T.'s dominance in domestic and overseas telephone service, exclusion of the manufacturers would give A.T.&T. overwhelming control of any system made up solely of carriers.

The FCC, meanwhile, went ahead and directed nine international carriers, led by A.T.&T., to report by 13 October on joint development and operation of a system. Simultaneously, the FCC rejected G.E.'s petition to include equipment manufacturers. Three days later, A.T.&T. and the National Aeronautics and Space Administration signed an agreement for the launching of two to four satellites next year at an estimated cost to A.T.&T. of \$6 million each.

Testifying before several congressional committees, FCC Chairman Newton Minow indicated that the door had not been shut irrevocably against the manufacturers. He said that they "may well be" permitted to participate eventually, and he declared that the FCC would not tolerate domination by any one firm.

General Electric, however, showed no inclination to tie up substantial assets on the basis of this assurance. With its current prospects snuffed out, it wrote the FCC on 22 September that it was dissolving Communications Satellites, Inc., which it had formed to pursue space communications activities. The unit, according to a company official, had approximately 100 employees, and they have been reassigned to other activities.

### Soviet Medical Exhibition

A Soviet medical exhibition, which had been scheduled to begin a 60-day U.S. tour on 23 September [*Science* 134, 932 (29 Sept. 1961)], and an American transportation exhibition which was to open in Moscow on the same date, have been indefinitely postponed by the Russians.

On 1 September the U.S. State Department asked to review the Russian contract with a Brooklyn construction company handling the medical exhibition, in order to ensure that liaison men assigned to Soviet exchanges in the U.S. will be paid. Soviet representatives stated that there was no formal agreement between the two countries allowing the right of review, and charged the U.S. with "interference."

The Soviet exhibition, still crated, is under the custody of U.S. Customs officials. The American exhibition, partially completed at the time of the postponement, is now being dismantled and crated in the event the show is ordered to continue to Stalingrad.

## Announcements

Designs have been completed for an **aluminum research submarine**, which will be used by Woods Hole Oceanographic Institution as a part of a research program sponsored by the Office of Naval Research.

The 51-foot, battery-powered *Aluminaut*, equipped with sonar, television cameras, and mechanical devices to retrieve specimens, will have an 80-mile operating range and will be capable of descending to 15,000 feet. It will be used initially to study submarine canyons, the edge of the continental shelf, and the daily vertical migration of marine animals. Designed by Reynolds International, Inc., a subsidiary of Reynolds Metals Company, the three-man craft will be constructed by General Dynamics Corporation's Electric Boat Division. Launching is scheduled for 1963.

**Visiting professors in astronomy** are currently available for 2-day intervals during the current academic year through a program established by the American Astronomical Society. Educational institutions wishing to utilize this service should write the Visiting Professors Committee at one of the following addresses:

*East:* Franklyn M. Branley, The American Museum-Hayden Planetarium, 81 St. and Central Park West, New York 24.

*Middle West:* Victor M. Blanco, Case Observatory, Taylor & Brunswick Roads, East, Cleveland 12, Ohio.

*West:* Seth B. Nicholson, Mount Wilson and Palomar Observatories, 813 Santa Barbara St., Pasadena, Calif.

A pamphlet on **family food stockpile for survival** has been published by the Department of Agriculture. The booklet, available free of charge, includes information on how to store and how often to replace food stockpiles; it also lists equipment needed for emergency cooking and gives sample menus for a reasonably balanced diet. (Office of Information, USDA, Washington 25, D.C. Order No. HG-77)

Airlie Foundation, a **conference center** in a rural setting about 40 miles from Washington, D.C., has opened for the use of educational, research and governmental organizations. The center is located on a 1200-acre estate and has facilities for 100 overnight

guests. A variety of conference rooms can accommodate small and large meetings. In addition, facilities for recreation are provided. The foundation is incorporated on a nonprofit basis. Among the organizations planning conferences at Airlie are the Brookings Institution and the Federation of American Societies for Experimental Biology. (Airlie Foundation, Warrenton, Va.)

Copies of a preliminary directory of the **biomedical library resources** in the Pacific area, presented at the 10th Pacific Science Congress (Honolulu, 22 Aug.-2 Sept.), are available on request. (National Library of Medicine, U.S. Public Health Service, Washington 25, D.C.)

**Cytologists and geneticists** who may have previously sent reprints of their work to Friedrich Mechelke, a German cytologist formerly with the Institut für Kulturpflanzenforschung in East Germany, are requested to send new copies. In the process of leaving East Germany in the last days before the closing of the barriers, Mechelke lost all of his scientific data, literature, and personal possessions. (Friedrich Mechelke, c/o Dr. J. Straub, Max-Planck-Institut für Züchtungsforschung, Köln-Vogelsang, West Germany)

### Courses

A 2-week postgraduate course in **allergy** is being offered from 5 to 16 March 1962 by the University of Pennsylvania. The course consists of a review of the basic principles of immunology and allergy as applied to clinical practice, with emphasis on the methods of diagnosis and management of allergic patients. Enrollment is limited. Tuition: \$175. (George Blumstein, c/o Temple Medical Center, Philadelphia 40, Pa.)

An 8-week course on **science research in contemporary Europe**, sponsored by the State University of New York and the Experiment in International Living, will be held during the summer of 1962. Participants will visit research institutions, oceanographic stations, zoological and botanical gardens, and museums in Rome, Venice, Genoa, Monaco, Marseilles, Paris, Brussels, The Hague, and London. (Walter Lerner, State University of New York, College of Education, Geneseo)

An advanced training course and an international conference on **modern techniques in activation analysis**, sponsored by the International Atomic Energy Agency, the U.S. Atomic Energy Commission, and the Agricultural and Mechanical College of Texas, will be held at the college in December. The course, from 4 to 14 December, will include lectures on fundamentals, sample preparation, sources, radiochemistry, present applications, and probable future developments. Laboratory experiments will utilize representative activation sources, multichannel analyzers, and computers. Requirements for admission are a B.S. degree and a minimum of 1 year in research. The \$250 tuition charge will include registration for the subsequent conference, to be held on the 15th and 16th. A \$15 registration fee will be required for conference participants who do not enroll in the course. (Richard E. Wainerdi, Activation Analysis Research Laboratory, A & M College of Texas, College Station)

#### Grants, Fellowships, and Awards

Applications are being accepted for **dental research and teaching traineeships** in the basic sciences, periodontology, and in the treatment and rehabilitation of the cleft palate patient. The traineeships, supported by the U.S. Public Health Service, carry minimum stipends of \$5000. (Chairman, Traineeships Committee, University of Pennsylvania School of Dentistry [basic science and cleft palate]; Dean, Graduate School of Medicine, 237 Medical Laboratories, University of Pennsylvania, Philadelphia 4 [periodontology])

A limited number of **conference travel grants** are available to **social scientists** who wish to attend the *Congress of International Economic Association*, the *International Congress on Economic History*, or the *International Institute of Administrative Sciences*. Eligibility requirements include residence in the United States, a Ph.D. degree or its equivalent, current work in fields relevant to the conference program, and publication of a significant contribution to research in the field. Deadline: **15 January 1962**.

Travel grants are also available for attendance at other meetings, held outside the U.S., Canada, or Mexico, that are open to participation by social

scientists from several nations. Requests for these grants may be submitted at any time. (Social Science Research Council Grants, 230 Park Ave., New York 17)

Eight 1962-63 fellowships in **industrial medicine** are being offered by the U.S. Atomic Energy Commission. The fellowships are open to physicians who have completed at least 1 year of internship and who are licensed to practice medicine in the U.S. The program consists of 2 years of academic work followed by an optional year of field training. Deadline: **1 January 1962**. (Henry A. Blair, AEC Fellowships, University of Rochester School of Medicine, Rochester 20, N.Y.)

#### Meeting Notes

An international postgraduate symposium on **alcohol and civilization** will be held from 11 to 13 November at the University of California Medical Center. The symposium will include sessions on alcohol's effects on the body, behavior, and emotion; its part in contemporary culture; and its relation to individual and community responsibilities. (Continuing Education in Medicine and Health Sciences, University of California Medical Center, San Francisco 22)

A symposium on the role of **science in the development of natural resources**, with particular reference to Iran, Pakistan, and Turkey, will be held from 8 to 13 January 1962 in Lahore, Pakistan. The symposium, sponsored by the Economic Committee of the Central Treaty Organization, will cover hydrology and water conservation; agriculture and soil; forestry; plant products; animal health; fuel; and public health, with emphasis on the scientific approach. Participants will be invited to consider which of these main fields are of interest to the afore-mentioned countries. Deadline for submission of titles and brief abstracts: **30 November**. (M. L. Smith, CENTO Institute of Nuclear Science, P.O. Box 1828, Tehran, Iran)

The second conference on **experimental clinical cancer chemotherapy** will be held in Washington, D.C., from 2 to 3 November. (Kenneth Flieger, Information and Education Section, National Cancer Institute, Bethesda 14, Md.)

An international conference on **palynology**, covering modern and fossil pollen, spores, and related microorganisms, will be held at the University of Arizona from 23 to 27 April 1962. The conference will include symposiums and round-table discussions. Deadlines: titles, **16 October 1961**; abstracts, **2 January 1962**; discussion topics, **1 December 1961**. (International Conference, Geochronology Laboratories, University of Arizona, Tucson)

The 1961 **National Youth Conference on the Atom**, intended for selected high school teachers and students, will be held from 9 to 11 November in Chicago. The conference will include discussions on fundamental particles, the physical and biological sciences and their relationship, electric power from nuclear fusion, and the use of radiation and radioisotopes; a symposium for teachers; and tours of the Museum of Science and Industry, Argonne National Laboratory, and Dresden Nuclear Power Station. (H. Richard Silver, Bozell & Jacobs, Inc., 230 Park Ave., New York 17)

A 1-day symposium on **planned parenthood and social action** will be held on 24 October in New York. The meeting deals with "the process of social change in which a matter of personal and private concern, such as birth control, becomes a subject for affirmative public policy." (Winfield Best, Planned Parenthood Federation of America, 501 Madison Ave., New York 22)

The location of the New York Academy of Sciences' **conference on the cervix**, to be held from 7 to 9 December, has been changed from the Barbizon-Plaza Hotel to the Henry Hudson Hotel, New York City. (Alfred B. Kupferberg, Ortho Research Foundation, Raritan, N.J.)

An open symposium on **nuclear education** will be held on 27 October in Philadelphia. The meeting, sponsored by Nuclear Industries, Inc., will cover nuclear training at high school, college, and graduate levels as well as the programs offered by various government agencies. Admission is free. (Grafton D. Chase, Philadelphia College of Pharmacy and Science, Philadelphia)

A research conference on **molecular structure and biochemical reactions** will be held in Houston, Texas, from 4 to 6



December 1961. The conference is open to the public, but advance registration is required. (W. O. Milligan, Robert A. Welch Foundation, P.O. Box 1892, Houston 1)

## Scientists in the News

Recent staff appointments at North Carolina State College of Agriculture and Engineering:

**William M. Roberts**, head of the animal industry department's dairy manufacturing section, has been named head of the college's recently formed department of food science and processing.

**Harold A. Lamonds**, project director of the college's nuclear reactor since 1957, has been named head of the new department of nuclear engineering.

**John E. Nordlander**, of Massachusetts Institute of Technology, and **Paul C. Moews, Jr.**, of the University of Michigan, have become instructors in organic and inorganic chemistry, respectively, at Western Reserve University.

Recent staff appointments at the University of Oregon:

**Sidney A. Bernhard**, chief of the physical chemistry section at the National Institute of Mental Health, will become an associate professor of chemistry in the university's institute of molecular biology.

**Marshall Fixman**, senior fellow in the chemistry department of the Mellon Institute, will become director of the university's new institute of theoretical science.

**Roger B. Fuson**, of the Montana Deaconess Hospital; **Charles A. Miller**, of Wabash College; and **Trygve W. Tuve**, of the National Institute of Arthritis and Metabolic Diseases, have been appointed staff members of the Research Grants Branch in the National Institutes of Health's Division of General Medical Sciences.

Recent staff resignations at the Oak Ridge Institute of Nuclear Studies:

**Adrian H. Dahl**, of the Special Training Division, has become professor of radiation biology at Colorado State University School of Veterinary Medicine.

**David S. Anthony**, of the University Relations Division, has returned to the University of Florida after a 1-year leave-of-absence from his post as associate professor of chemistry.

The following nuclear engineers have been appointed vice presidents of Millitron, Inc. (Irwin, Pa.), a newly established firm specializing in heat measurement and nuclear instrumentation:

**Vincent G. Shaw**, general manager; founder and first president of Shaw Instrument Corporation, and inventor of the Shawmeter, the first two-color pyrometer.

**Andrew J. Pressesky**, optical specialist; former instrument designer for Atomic Energy of Canada, Ltd.

**Basil M. Lide**, one of the developers of nuclear instrumentation and radiation monitoring equipment for the *Nautilus*.

**Sidney W. McCuskey**, Kerr professor of mathematics and astronomy and head of the astronomy department at Case Institute of Technology, has received the third annual Case achievement award. McCuskey is also director of the Warner and Swasey Observatory and the Nassau Astronomical Station at the institute.

**Clifford K. Beck**, nuclear physicist, has been appointed deputy director of regulation for the U.S. Atomic Energy Commission. He was former assistant director for facilities licensing in the commission's Division of Licensing and Regulation. **Robert Lowenstein**, social scientist and former acting director of the licensing division, has been appointed director.

Recent staff appointments at the University of Kentucky Medical Center:

**Tihamer Z. Csaky**, of the University of North Carolina School of Medicine, has been named professor and the first member of the newly formed department of pharmacology in the center's College of Medicine. **Rene Menguy**, of the University of Oklahoma Medical Center, has been named associate professor of surgery in the college.

**May Sanders**, a former staff member at the University of Georgia School of Nursing, has been named associate professor and assistant dean of the center's College of Nursing.

**Harlan W. Northrup**, formerly with Battelle Memorial Institute, has been elected president of Technical Aid Service, Inc., a firm that specializes in gathering and assimilating information for research scientists.

**L. C. Widdoes** has been named director of research for Petrolite Corporation, St. Louis, Mo. He was former president of Internuclear Company, a subsidiary of Petrolite.

**Luis F. Leloir**, biological chemist at the University of Buenos Aires, has been named Dunham lecturer at Harvard University for the current academic year.

**James K. Shafer**, chief of the Public Health Service's Division of Community Health Practice, has been named director of health services in the PHS Office of Civil and Defense Mobilization. He succeeds **W. Palmer Dearing**, who retired to become the first executive director of the Group Health Association of America.

**L. Wilson Greene**, chief technical adviser at the U.S. Army Chemical Research and Development Laboratories, has received the Army's Meritorious Civilian Service decoration. Greene retired last month after 32 years of government service.

Recent staff appointments at the University of Rochester:

**Lawrence Raisz**, assistant professor at the State University of New York College of Medicine, has been named associate professor of pharmacology and medicine in the university's School of Medicine and Dentistry.

**Aser Rothstein**, professor of radiation biology in the Medical School, has been named vice chairman of the department and associate director of the university's atomic energy project.

**Max Jordan** and **William Grant**, of the U.S. Department of Agriculture, have been appointed cooperative employees of the USDA and the Arkansas Agricultural Experiment Station.

## Recent Deaths

**Sterling Ely**, 62; research chemist and Washington representative for the Union Carbide Corporation; 15 Aug.

Sister **Mary E. O'Hanlon**, 79; emeritus professor of botany at Rosary College, River Forest, Ill.; 24 Aug.

**Burt H. Weston**, 80; refrigeration engineer and head of the engineering department of Columbia Technical Institute until his retirement in 1950; 15 Sept.



# Structure of Science

Philosophy of science, a separate discipline, meets philosophy proper on the question, "What exists?"

May Brodbeck

Ernest Nagel's treatise on the philosophy of science has, for some time now, been anticipated eagerly by all who are familiar with the high quality of his many essays in this area. The anticipation is thoroughly justified by this volume. The several fundamental issues in the logic of scientific explanation are here discussed extensively and in depth. The nature of scientific laws, of causality, of theories, and of explanation are considered not only in general, but also as these problems arise within specific contexts in physics, both classical and modern, in the biological and the social sciences, and in the study of history. The connections among these areas, that is, the various aspects of the problem of reduction, are treated in detail, as they should be, in a volume devoted to scientific explanation. Nagel is as scholarly and accurate about the relevant scientific and historical details as his readers have good reason to expect him to be, and that is scholarly and accurate indeed. As one would also expect, Nagel attends not only to the more technical issues in the philosophy of science, for instance, the nature of geometry and its relation to physics, but also to matters of more general interest. He discusses the various claims that have been made by laymen and by some scientists-turned-Sunday-philosophers about, say, the relation between the quantum theory and the thesis of scientific determinism, about the possibility of a science of man and society, and the implications these issues have for human freedom. In all such matters he is, to my mind, on the side of the angels. Patiently and carefully he exposes the fallacies and confusions of

those who willfully or ignorantly misinterpret and twist the findings of science in the interests of obscurantist special pleading.

If, gratifying as all this is, for many of us the book contains few surprises, that is hardly a relevant criticism. It is an excellent thing to have it all here, systematically worked out, in one volume. (A second is also promised.) Nor are we, after all, the audience that matters. Some lessons, some insights are peculiarly slippery and, it seems, must always be learned anew by each generation. Many of those lessons and insights will be found in this volume. Not the least is the general nature of the work. His book really is a study in the logic of science, not merely speculative commentary on how scientists contrive to make discoveries or to communicate with one another. This is wholly admirable. If I must dissent from Nagel on certain fundamental issues, this does not in the least affect my judgment that the work as a whole is a most valuable and considerable achievement.

## "Real" and "Exist"

The philosophy of science, unlike philosophy proper, takes our common-sense world of tables and chairs, the sun, the stars, and other people for granted. In other words, it assumes the common-sense realism of our everyday belief in the independent existence of an external world. Insofar as the philosophy of science concerns itself with questions about "reality," it is not with the status of ordinary perceptible material objects and their properties, but with those special entities which the scientist talks about but which we do not perceive. As Nagel points out, we

do not know that there are electrons in the same way or in the same sense that we know we have hands and feet. Since we don't, there is a problem. Are these entities real, like hands and feet, or are they speculative, as the existence of men on Mars is speculative, or are they merely fictions, not perhaps as unicorns are fictions but as being merely verbal or symbolic instruments useful to the scientist for explaining and predicting phenomena? The problems and the range of answers are familiar. The dialectic is subtle and complicated. Before a stand can be taken, the various connotations of "real" and "exist" must be teased out. To exist is to be observable: for the materialist, observable by more than one; for the dualist, a felt pain is as real as a chair. Again, to exist is to occur in one or more scientific laws. Or, to exist is to be a simple element of which everything else in some sense consists. These are some of the connotations that are relevant to clarifying and resolving the issue. Once such distinctions have been made, there may be little more that needs to be done in order to show how one may agree that there are indeed electrons, that they are real, yet agree too that there is an "ontological" difference between electrons and chairs or colors. This done with the care that it requires, one may well tend to echo Nagel's statement that the difference between the instrumentalist and realist views is simply a "preferred mode of speech." Yet given the grounds on which he makes this assertion and the scope he allows it, radical dissent is, I believe, required of anyone who wants to maintain our common-sense realism.

Inevitably, at some point philosophy proper which examines our common-sense assumptions will, implicitly or explicitly, be injected into the discussion. However, an adequate philosophical analysis must preserve our ordinary belief in the independent reality of tables and chairs and, once these are firmly anchored, then discuss the connection of electrons and neutrinos to this reality. An analysis that fails in the basic philosophical task has not eluded the verbal snares leading to the Pickwickian denial of an external world, whether that denial takes the form of straightforward idealism or of any of its instrumentalist variants. Unfortunately, just at this crucial juncture where the philosophy of science and philosophy proper meet, Nagel's analysis is weak and vacillating. We see our hands and

The author, professor of philosophy at the University of Minnesota, Minneapolis, reviews here *The Structure of Science* by Ernest Nagel (Harcourt, Brace and World, New York, 1961, 618 pp. \$7.50).

feet, but not molecules or psi-functions. Accordingly, Nagel distinguishes theoretical laws or "theories," groups of statements about these unobservable entities, from "experimental" laws which, not necessarily based on laboratory experiment, state connections among observable things and their properties. The distinction is familiar and important. Nagel agrees that it is important, but holds also that it is "vague." Here begins, for me, the elusiveness of Nagel's discussion. *On the one hand*, we are told that with each experimental concept a definite overt procedure is associated, endowing its laws with "determinate empirical content," while this is not true of theoretical terms or of the statements in which they occur. Repeatedly, reference is made to the "facts of observation." We also have extended comment, with good illustrations, on the significant circumstance that our explanatory theories may change while the experimental laws remain the same, nor are the latter "contingent upon the fate" of the former. These comments support the thesis that hands and feet are "ontologically" different from electrons and molecules. *On the other hand*, Nagel also insists that the distinction between what is and what is not observable, though important, is only one of degree. So too, therefore, is the distinction between changing "theoretical" and stable "experimental" laws. This amounts to saying that there really is only a difference in degree between hands and feet and electrons. It follows, for Nagel, that the distinction between the instrumentalist and the realist views with respect to hands and feet as well as with respect to electrons is only verbal. The point at which we have merely a "preferred mode of speech" is thus pegged at the level, not of the problematic entities of theory, but down to ordinary material objects and their properties, like boiling point and temperature.

### Dewey's Influence

Though he conscientiously catalogs the weaknesses of instrumentalism and the strengths of realism, Nagel ultimately is unable to free himself of instrumentalism. (John Dewey was an early, strong, and, as must now be seen, ineradicable influence on his thought.) The explicit snare in his case, trapping him, one can't help feeling, into instrumentalism, is the notion of "implicit

definition." Doing no harm in mathematical, purely formal contexts, it leads to catastrophe when applied to descriptive systems by generating the illusion that uninterpreted marks on paper, symbols that have not been tied to observable referents, nevertheless have descriptive meaning. Nagel is aware of this illusion and its dangers when discussing the difference between pure and applied geometry and the problem of the relation of geometry to physics. Indeed, without this awareness, that issue cannot even be approached intelligibly, let alone discussed as excellently as Nagel does. Yet he carries over the notion of "implicit definition" from contexts where it is relatively innocuous to contexts where it is far from innocuous. Not only the theoretical concepts, but the experimental descriptive concepts are all said to be "implicitly defined" by the statements in which they occur. As is well known and lucidly discussed in detail by Nagel, the terms of a theory are only "partially coordinated" to experimental concepts. This means that not the individual terms, like mass- or velocity-of-a-molecule, but only certain arithmetical functions of combinations of them, like momentum, are logically tied to experimental concepts like pressure. If the term were not compromised beyond repair, it would perhaps do no harm to say that the uncoordinated individual terms of a theory were given "meaning" or "implicitly defined" by its axioms. When, however, this notion is carried over to the experimental concepts, which are the source and basis of such descriptive meaning as the theory has, instrumentalism is unavoidable.

Nagel justifies extending the notion of "partial" meaning to the experimental concepts by leaning in part on the argument from the possibility of alternative definitions when a term occurs in several well-confirmed laws. This circumstance, so the argument goes, means that each law partially defines the terms occurring in it. But if at any given time, one of the observable phenomena associated with the concept were not chosen as the defining property, then we could never make any empirical statements about that concept. Apparently aware of this danger of either unintelligibility or vacuity, Nagel concedes that when several procedures are available one of them is chosen as the defining property. His chief argument therefore apparently rests on the use of the real-number system and of

such idealized notions as point-masses, while we measure only discontinuous magnitudes and actual bodies. For Nagel, this apparatus puts macroscopic Newtonian theory on a par logically with quantum mechanics, the experimental concepts in the same boat with theoretical ones, that is, velocity of a car in the same boat with velocity-of-a-molecule. But what is partially coordinated in such cases is part of the *logical* apparatus, a real number to a class of rationals; a point with zero dimension to the moon. The issue, however, concerns not the logical but the *descriptive* terms, like mass-of-a-molecule and temperature. It can therefore only be clarified by first assuming once and for all the partial interpretation of the logical, arithmetical concepts, which are shared by theoretical and experimental statements alike, and then by inquiring into the difference between electrons and billiard balls, for there is *still* a difference and that is still a problem.

At the root of Nagel's inability to embrace an unequivocal realism with respect to hands and feet is his intense dislike, inherited from Dewey, of any "given" element in experience, any self-contained noninferential knowledge that may serve as the basis for all knowledge. Though tables, like colors, are observable, unlike colors they are not at any one instant wholly observed. Thus about tables and thermometers it is possible to raise a question, though not, intelligibly, about colors and sounds. Fearful of that *bête-noire* of all instrumentalists and idealists, sense-data, he rehearses, albeit in an off-hand and half-hearted way, the conventional arguments against the translatability thesis. He concludes, conventionally enough, that because the "translation" proposed is not practically feasible, the thesis has no justificatory force. So the question of the physical reality of raindrops and temperature, like that of molecules and kinetic energy, becomes merely a matter of a "preferred mode of speech," for neither temperature nor, *ipso facto*, kinetic energy can have meaning apart from the system in which they are inextricably bound.

Thus, despite the many illuminating things Nagel says about the structure of theories, his discussion of the status of the theoretical entities founders and fails at the juncture where philosophy proper meets the philosophy of science. With the particles left unanchored to an unequivocally external reality, the whole edifice crumbles. It crumbles because

there remains no truth or falsity which does not depend upon either a shifting usefulness or a viciously regressive coherence. Without this self-contained truth, the notions of testing a theory and of what is and what is not evidence become unintelligible. Fortunately, many issues in the philosophy of science are remote from this delicate philosophical underpinning. Despite, therefore, what seem to me the weaknesses of this volume on such fundamentals, it can remain an admirable and distinguished contribution.

## Russian Anthropology

**The Ancient Culture of the Bering Sea and the Eskimo Problem.** S. I. Rudenko. Translated by Paul Tolstoy. Henry N. Michael, Ed. (Anthropology of the North: Translations from Russian Sources, No. 1.) Arctic Institute of North America and University of Toronto Press, Toronto, Canada, 1961. iii + 186 pp. Illus. \$3.

In a field as inadequately served as anthropology has been in this respect, it is indeed a pleasure to hail the inauguration of a Russian translation series as admirably conceived and competently executed as this new project of the Arctic Institute of North America. The project, which is supported by the National Science Foundation, may well serve as a model for comparable projects in other disciplines. Congratulations are in order for the organizer, Henry B. Collins, and for the editor, Henry N. Michael, who combines linguistic proficiency and knowledge of the subject matter with editorial experience. Paul Tolstoy's translation of this first volume sets a high standard.

The work selected for the initial publication is a basic contribution to Eskimo archeology by one of the outstanding senior Soviet archeologists. It records the results of field research (in 1945) on the Siberian side of the Bering Strait. Rudenko's investigations were in the nature of a reconnaissance: locating and testing sites, gathering collections, and carrying out some limited excavation at the most promising points. This excellent report describes the work, the sites, and the sizable collection of specimens; it is copiously illustrated.

The most important result was the discovery at Uelen, near East Cape, of a major site of the earliest (Okvik) stage

of the classic Bering Sea culture sequence. Remains of all succeeding stages were identified at one point or another on the coast of the Chukchi Peninsula, but no earlier traces were found—a matter of some interest to proponents of an Asiatic origin for Bering Sea Eskimo culture. The report concludes with a discussion of Eskimo origins, based on the author's interpretation of the evidence provided by the harpoon and skin boat complex, art styles, and composite implements. Rudenko justly demolishes the efforts of Western scholars to derive the Eskimo from interior Siberia but on the other hand demonstrates the presence of southern parallels to Eskimo culture. Somewhat carried away by his preoccupation with the latter, he sees the Eskimo as a group of migrants from insular southeast Asia, who arrived at a relatively late date and who intruded as an alien wedge into the Bering Sea region. However, this hypothesis lacks any foundation when other types of evidence are examined and has attracted no following, although it has served a useful purpose in drawing attention to the neglected problem of cultural relationships between the Eskimo area and the Pacific coast of Asia. I hasten to add that these speculations in no way detract from the solid value of Rudenko's report.

Additional volumes of this series, promised for the near future, will be eagerly awaited.

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## Regulatory Profession

### Science and Public Administration.

James L. McCamy. University of Alabama Press, University, 1961. viii + 218 pp. \$3.50.

Science and government are old partners who are now getting along very well together, due to a new profession, public administration, which was developed to serve as the channel of communication and to insure that scientists do not engage in government or government delve in science. Perhaps this is not exactly McCamy's thesis, but the scientist who reads his presentation may gain the above impression. McCamy presents the case for the specialized profession of the public adminis-

trator, the official who, by reason of special abilities and training, is capable of making the administrative decisions, once the scientists present him with the facts. To a degree the scientist who reads this book may be prejudiced by the subordinate role to which he is assigned and by the implication that the scientist is the technician to the administrator who takes the scientists' developments and decides how to use these in the world today.

Too much emphasis is given to establishing a gap between science and other cultural areas by developing the myth of science and the conflict with religion. These conflicting arguments are used to develop a need for a group other than scientists, rather than a group including scientists, to administer the problems science creates.

If McCamy is disturbed that the National Science Foundation gives, in his opinion, too little support to the social sciences, he may well be interested to know that some scientists feel otherwise. To the author of this book, science includes the social and behavioral sciences, and in fact there is a hint at times that he considers public administration a science involving knowledge and techniques as complicated as other recognized sciences.

While consideration is given to the organization and structure of science, there is a lack of a parallel discussion of the development of the public administrator, how one learns to make decisions on scientific facts, and on what basis and how one integrates scientific, political, and social facts. If it is true that "science creates the social problem for which the public administrator must recommend solutions," then the inquiring scientist-reader would like to know how these solutions are obtained by public administrators, so that he can weave these into the social monsters he is accused of creating.

There are some who write in a challenging manner to hold the reader's interest. Scientists may not agree with the role which McCamy has given them, but they will find his discussion of continued interest; interspersed between ideas with which they will take issue are many thoughts which will merit both further study and support. The book is not a handbook on how to administer a public science organization, but rather something for both scientists and administrators to "chew on."

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## Political Ornithology

**Bird Study.** Andrew J. Berger. Wiley, New York, 1961. 400 pp. Illus. \$9.

*Bird Study* is designed, according to its preface, to be used in a one-semester ornithology course for liberal arts students. Such students have seldom had extensive biological training, and their approach to ornithology can hardly be expected to be highly sophisticated. It is, therefore, a surprise to find that this text contains caustic criticisms of other workers or schools of workers (particularly in the chapters on behavior and systematics), "witty" jibes at ideas not held by the author, and facetious statements meant, apparently, to demolish the opposition. Little evidence for the views accepted or rejected is given, and students may thus be left with a biased attitude toward widely held ideas which they actually are unprepared to judge fairly. It is certainly advisable to expose beginning students to the different interpretations and approaches within a science, and, after the conflicting evidence has been fairly covered, it is perhaps even permissible for an author to indicate his own views. But care must be exercised lest students be led to accept these views uncritically. Sarcastic witticisms are out of place in such a text.

Personal bias is especially manifest in the chapter on conservation (which covers much material having little to do with ornithology), where federal farm policies are roundly lashed and a political cartoon from *Life* magazine is included. It is unusual to learn so much of an author's political position by reading his ornithology text.

The organization of the material is somewhat disturbing; portions of a subject may be covered in several different chapters. Thus anatomy appears in both the first and ninth chapters, and behavior theory is treated in at least four chapters, only one of which is entitled behavior.

The discussion of ethology includes some misleading sections; for example, displacement activities are poorly treated and mistakenly related to vacuum activity; a conflict between a "social-stimulation school" and a "hostile-behavior school" is emphasized, though it hardly exists in fact (fighting is considered by Berger as a device for "mutual stimulation," but it is difficult to see why he feels this automatically cancels its hostile nature).

A good introductory ornithology text

has long been needed, and this one is certainly superior to its predecessors for it gives more extensive coverage to newer aspects of the subject and has an approach more in tune with modern evolutionary thought. It represents a welcome advance, but, unfortunately, not as great an advance as could be desired.

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## Sinews or Fat

**Educating Gifted Children.** Robert F. DeHaan and Robert J. Havinhurst. University of Chicago Press, Chicago, Ill., ed. 2, 1961. x + 362 pp. \$5.

This volume, one of several on its topic recently appearing, is a third larger than the first edition of only 4 years ago [reviewed in *Science* 126, 615 (1957)], and it well indicates the growth of interest in the subject. Concepts of giftedness are broadening; new to this edition are chapters on creativity and on "non-intellectual" talents, as in leadership and the arts. A fifth of the total school population is considered to deserve special attention, as superior in some respect. Selection should be on the basis of a variety of testings and other evidence, should begin early (so "creative rhythms" tests may be given in the first grade "to see how well a child could follow a definite dance pattern"), and should be reviewed periodically.

A new chapter on objectives and curriculum stresses adequate planning: the teachers of each local school district should study "the characteristics of the gifted children in its own schools . . . the lives of great men . . . and our national purposes and the desires of humanity all over the world." The values of and research support for acceleration are recognized. But enrichment is "the key concept." A great variety of materials and methods are suggested for use in the regular classroom, in special groups or classes, or in the community—the book ends with a description of a "junior theatre," with a professionally trained director, to serve children with dramatic talent from the third grade through high school. Sample programs for the gifted from elementary school through college are described.

No mention was noted of possible values of work in the total education

of the gifted, though a relevant summer job has been part of some programs. The chapter on evaluation omits career and other outcomes in adult life, though Terman and others have shown the importance of such follow-ups.

But, in sum, the volume inclusively reports current experimenting in this field, much of it generously subsidized. Indeed, the necessity might be inferred of an elaborateness of approach beyond the ordinarily feasible, or perhaps sometimes desirable. Might an able youngster, eager to get ahead with his education and into his life-work, need help in avoiding some of the plentitude of testings and guidings and enrichings! Might a crowded school, with no Carnegie grant in sight, refuse to consider any program for its gifted! I believe that there can be lean vigorous programs, realistic about such problems, and that more consideration should be given them.

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## Glucuronic Acid

**Chemistry of Drug Metabolism.** William H. Fishman. Thomas, Springfield, Ill., 1961. xvii + 235 pp. Illus. \$10.50.

The study of drug metabolism is an important field with an extensive and expanding literature; therefore current reviews are of considerable value to those engaged in research on the metabolism of drugs and toxic chemicals. William H. Fishman, the author of this monograph, is well known for his significant contributions to the study of metabolic conjugation of drugs with glucuronic acid and the biochemical role of  $\beta$  glucuronidase. His discussion of this topic, which comprises one-half of the book, is a welcome and timely reference. He considers in detail the chemistry of glucuronic acid, its role in carbohydrate metabolism, and its physiological and pharmacological significance. He relates drug metabolism to normal biochemical mechanisms and presents some interesting conclusions concerning the role of conjugation in solubility, penetration, and the transport of active substances to target organs. The enzymatic functions of  $\beta$  glucuronidase are also discussed in this context.



The remainder of the book deals with the metabolism of drugs by pathways other than glucuronic acid conjugation, and it might be considered somewhat sketchy and incomplete—for example, epinephrine metabolism is covered in two short paragraphs. Perhaps it would have made for easier reading had the author started with general mechanisms of drug metabolism rather than with specific examples. Despite these minor criticisms the book should be received with appreciation by those engaged in research on the metabolism of organic chemicals and drugs, both for its content and for its excellent bibliography.

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## Controlling Hazards

**Radioactive Wastes.** Their treatment and disposal. J. C. Collins, Ed. Wiley, New York; Spon, London, 1960. 239 pp. Illus. \$8.

The benefits of atomic energy cannot be attained without accepting the risks. Radioactive wastes provide one insidious hazard. This timely book is one of the most complete volumes yet published dealing solely with that waste problem. Eight authorities combined their talents in this excellent, highly technical but easily readable symposium volume, and they summarize work throughout the world. Of course they concentrate on Great Britain's problems and the solutions so far obtained there.

The authors are properly conservative but realistic. They offer four general precepts or guides: (i) disposal is ultimate only after radioactive decay, (ii) dispersed radioisotopes may be re-concentrated to hazardous levels, (iii) carefully scaled-up experiments are necessary before full-fledged disposal, and (iv) extensive and exhaustive environmental sampling and evaluation are necessary to document safe operation, to provide factual knowledge of the processes involved, and to relate theory, experimental data, and practice.

Half the book deals with fundamentals of radioactivity (the nature, hazards, measurement) rather than with disposal or disposal practice. That part provides a complete background, however, and is well keyed to waste disposal. Many examples enlighten the subject.

The section concerned with actual disposal describes thoroughly the theory

and practice of disposing of gaseous wastes into the atmosphere, burying solid waste on land and in the sea, and disposing of liquid waste in rivers and oceans. Numerous case histories and experiences are cited. Disposal of liquids in the ground, a dominant practice in some large-volume separation plants in the United States, is treated only briefly, and only brief mention is given to problems of long-term storage of wastes in tanks, on ceramics or other materials, until adequate decay occurs. For the length of time involved in this process, no container can be guaranteed corrosion- or fail-proof and no radioactive ceramic or other fixation product guaranteed unleachable. Hence the earth features that regulate waste behavior need be known.

The experience recounted here, because it reflects the waste disposal philosophy of the United Kingdom Atomic Energy Authority, makes their practices well worth studying. More discussion of the long-term and international aspects may well be warranted.

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## New Books

### Mathematics, Physical Sciences, and Engineering

**Analytical Elements of Mechanics.** vol. 2, *Dynamics*. Thomas R. Kane. Academic Press, New York, 1961. 353 pp. Illus. \$6.25.

**Atlas of the Universe.** H. E. Butler, Ed. Nelson, New York, 1961. 226 pp. Illus. \$9.95.

**Ballistic Missile and Space Vehicle Systems.** Howard S. Seifert and Kenneth Brown. Wiley, New York, 1961. 538 pp. Illus.

**Basic Concepts of Physics.** Chalmers W. Sherwin. Holt, Rinehart, New York, 1961. 421 pp. Illus. \$6.50.

**Calculus of Finite Differences.** Charles Jordan. Chelsea, New York, ed. 2, 1960. 673 pp. \$6.

**Combustion, Flames and Explosions of Gases.** Bernard Lewis and Guenther von Elbe. Academic Press, New York, 1961. 750 pp. Illus. \$22.

**Concepts of Mass.** In classical and modern physics. Max Jammer. Harvard Univ. Press, Cambridge, Mass., 1961. 230 pp.

**Dyeing of Cellulosic Fibres and Related Processes.** S. R. Cockett and K. A. Hilton. Academic Press, New York, 1961. 430 pp. Illus. \$12.

**Explosion Studies of Continental Structure.** Publ. 622. John S. Steinhart and Robert P. Meyer. Carnegie Institution of Washington, Washington, D.C., 1961. 422 pp. Illus. Paper, \$2.50; cloth, \$3.

**Geology of the Arctic.** vols. 1 and 2. Gil-

bert O. Raasch, Ed. Univ. of Toronto Press, Toronto, Canada, 1961. 1210 pp. + maps. Illus. \$25.50.

**Introduction to Chemical Engineering.** L. Bryce Andersen and Leonard A. Wenzel. McGraw-Hill, New York, 1961. 376 pp. Illus. \$9.50.

**An Introduction to Information Theory.** Fazlollah M. Reza. McGraw-Hill, New York, 1961. 517 pp. Illus. \$13.50.

**Lectures on the Calculus of Variations.** Oskar Bolza. Chelsea, New York, ed. 2, 1961. 280 pp. Illus. Paper, \$1.19; cloth, \$3.25.

**Mathematics in the Making.** Lancelot Hogben. Doubleday, Garden City, N.Y., 1960. 320 pp. Illus. \$9.95.

**A Modern Introduction to Organic Chemistry.** William B. Smith. Merrill, Columbus, Ohio, 1961. 271 pp. Illus.

**Namenreaktionen der Organischen Chemie.** Ein Beitrag zur Terminologie der organischen Chemie, Biochemie und theoretischen organischen Chemie. Helmut Krauch and Werner Kunz. Hüthig, Heidelberg, Germany, 1961. 591 pp. Illus. DM. 46.

**New Thinking in School Mathematics.** Organization for European Economic Cooperation, Paris, 1961. 246 pp. \$2.50.

**The Organic Chemistry of Boron.** W. Gerrard. Academic Press, London, 1961. 318 pp. Illus. \$9.

**Pressurized Packaging.** Aerosols. A. Herzka and J. Pickthall. Academic Press, New York; Butterworth, London, ed. 2, 1961. 520 pp. Illus. \$15.

**Principles and Applications of Paper Electrophoresis.** Ch. Wunderly. Elsevier, New York, 1961 (order from Van Nostrand, Princeton, N.J.). 253 pp. Illus.

**Programming and Coding for Automatic Digital Computers.** G. W. Evans and C. L. Perry. McGraw-Hill, New York, 1961. 261 pp. Illus. \$9.50.

**Progress in Cryogenics.** vol. 3. K. Mendelssohn, Ed. Academic Press, New York, 1961. 177 pp. Illus. \$8.

**Quantitative Organic Microanalysis.** Al Steyermark. Academic Press, New York, ed. 2, 1961. 682 pp. Illus. \$16.50.

**The Rare Earths.** F. H. Spedding and A. H. Daane, Eds. Wiley, New York, 1961. 652 pp. Illus. \$14.75.

**The Royal Society International Geophysical Year Antarctic Expedition, Halley Bay, Coats Land, Falkland Islands Dependencies, 1955-1959.** vol. 1. Introductions. Aurora and airglow. Geomagnetism. Sir David Brunt, Ed. Royal Society, London, 1960. 420 pp. Illus. \$23.

**Space Power Systems.** Nathan W. Snyder, Ed. Academic Press, New York, 1961. 649 pp. Illus. \$6.

**Stability in Nonlinear Control Systems.** Alexander M. Letov. Translated from the Russian by J. George Adashko. Princeton Univ. Press, Princeton, N.J., 1961. 330 pp. Illus. \$8.50.

**Theory of Elasticity.** V. V. Novozhilov. Translated from the Russian by J. K. Lusher. Pergamon, New York, 1961. 460 pp. Illus. \$12.50.

**Transactions of the Second Prague Conference on Information Theory, Statistical Decision Functions, Random Processes.** Publishing House of the Czechoslovak Acad. of Sciences, Prague; Academic Press, New York, 1960. 843 pp. \$22.

# Alkaline Phosphatase in Human Sera and Placentae

Starch gel electrophoresis reveals many phosphatase components including a polymorphism in placentae.

Samuel H. Boyer

Heterogeneity of nonspecific human serum alkaline phosphatase has been demonstrated by electrophoresis on starch granules (1), by cellulose chromatography (2), and by immunologic means (3). No more than three types of phosphatase have been observed by any one method. Our observations indicate that at least 16 bands of nonspecific alkaline phosphatase activity are evident in human sera following electrophoresis on hydrolyzed starch gel (4), although not all occur in a single individual. The purposes of this investigation have been, firstly, to describe the influence of disease, pregnancy, and ethnic origin upon the appearance of certain zones of enzyme activity; secondly, to explore by other methods the validity of the observed heterogeneity; and thirdly, to test the hypotheses that heterogeneity of the serum phosphatases peculiar to pregnancy reflects diversity of placental phosphatase and that such diversity is under simple genetic control.

Vertical starch gel electrophoresis was performed with a discontinuous buffer system (5) at 4°C. Zones of enzyme activity were developed directly on a longitudinal gel slice by using a solution containing 0.05-percent beta-naphthyl sodium phosphate, 0.005M MgSO<sub>4</sub>, 0.05-percent fast blue RR salt, and 0.06M pH 9.7 sodium borate-boric acid buffer (6).

The nonspecific alkaline phosphatase activity observed after electrophoresis has been arbitrarily divided into A, B, C, D, E, and F zones. The most rapidly migrating zone, A, lies immediately cathodic to the transferrin C band while the D zone centers about the

area of the haptoglobin 1-1 band. Examples of all except the uncommon E zone are shown in Fig. 1. Further diversity exists within zones such that two distinct bands have been observed in the A zone, two in the B zone, three in the C zone, five in the D zone, and three in the F zone. Among more than 700 sera examined, no one serum exhibited more than four zones with a total of eight distinct bands. Normal adults possess one or two C components and occasionally a faint F band. The single C component of childhood is uniformly slower than the C bands of adults.

With one exception, the A, B, and the more slowly migrating D components have been observed only in pregnancy. Such D components as are seen in pregnancy are uncommon and limited to Negroes. West African Negroes present much variation in the D zone. None of the phosphatase components of pregnancy have been seen prior to the 15th week of gestation. One or both of A and B zones are evident in almost all women by the 28th week of pregnancy. Once present these zones persist throughout gestation and disappear by the sixth postpartum week. The relative proportion of total serum alkaline phosphatase represented by the A and B components is approximately 0.50 and is sufficient to account for the major portion of serum alkaline phosphatase elevation developing during pregnancy. No evidence of A and B components appeared in the umbilical cord serum of more than 30 infants whose mothers possessed one or both these components. Components equivalent to A and B were not present in the serum of five pregnant rhesus monkeys.

The relative proportion of women

with A-zone activity was found to differ between American whites and American Negroes as shown in Table 1. Although numerous individuals with both A- and B-zone activity were detected, the only accurate distinction that could be simply made in every case was between A and "not-A." All "not-A" persons have B-zone activity. The differences between the two ethnic groups suggested the existence of a genetic polymorphism. Some confirmation of this possibility was provided by examination of sera from Nigerian women in the last trimester of pregnancy (Table 1). With the assumption that the presence of A-zone activity is determined by a dominant or co-dominant gene (the allele being responsible for zone B), gene frequencies can be computed with the further assumption of a Hardy-Weinberg equilibrium (Table 1). An independent estimate of American Negro hypothetical allele frequency can then be realized from the knowledge that the American Negro is approximately 70 percent West African and approximately 30 percent European in genetic origin (7). The frequency of the hypothetical A allele in American Negroes thus computed is

$$(0.7)(0.063) + (0.3)(0.319) = 0.140,$$

a figure in excellent agreement with the directly computed value of 0.134. Such argument suggests, although it does not prove, that the presence of A-zone alkaline phosphatase activity is simply inherited.

Among more than 120 patients, with a variety of disease and elevated serum alkaline phosphatase activity, only two had activity in other than C and F zones. Simple starch gel electrophoresis of nonspecific serum alkaline phosphatase does not, therefore, offer promise as a tool for differential diagnosis of disease.

The F zones may be  $\alpha_1$ ,  $\alpha_2$ , or  $\alpha_2$ - $\beta$ -globulin in conventional mobility. Several F components may be present after two-dimensional (paper-starch gel) (8) electrophoresis, although only one appears after simple migration on starch gel. The F zones appear to be related to the more rapidly migrating zones, since the appearance of D components is often accompanied by very slow F components. Tissue alkaline phosphatases are often associated with lipoproteins (9). Binding of certain serum alkaline phosphatases to large molecule lipids might account for the scant mobility of the F zone; however, neither F nor other zones of serum

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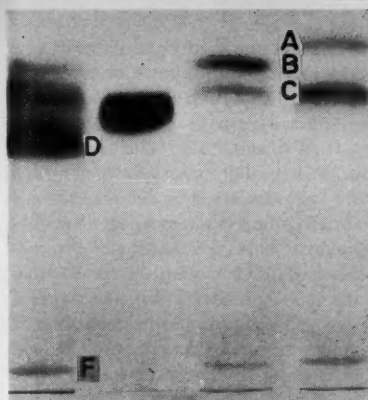


Fig. 1. Electrophoretic migration on starch gel of nonspecific alkaline phosphatase in four different sera. Migration was toward the top of page. The slots of sample insertion can be seen at the bottom of illustration. From left to right the serum patterns are BCD<sub>2</sub>, C-childhood, BC, and AC. F components are present, but poorly displayed, in all sera.

alkaline phosphatase behave as lipoproteins during ultracentrifugation in dense solution. Furthermore, *n*-butanol, which breaks the lipid-protein association (9), had no influence on serum-band or tissue-band alkaline phosphatase migration. A further characteristic of the F zone is its disappearance in concentrations of Rivanol (2-ethoxy-6,9-diaminoacridine lactate) which leave A, B, and C components unaffected.

Additional evidence for heterogeneity of serum nonspecific alkaline phosphatases in normal subjects is provided by cellulose chromatography. Sera from eight individuals with BC components and from one with AC components were pooled. Alkaline phosphatase was separated from the bulk of serum proteins by fractionation with Rivanol. Rivanol was removed by passage through G-75 Sephadex, and phosphatase was then concentrated on a 0.01M NaCl diethylaminoethanol-cellulose column. Phosphatase was eluted as a concentrate, dialyzed, applied to a triethylaminoethanol-cellulose column, and then eluted with a linear NaCl gradient. Eluted fractions from triethylaminoethanol cellulose were characterized by enzymatic assay and by their behavior during starch gel electrophoresis. The small amount of A-zone activity as well as previously undetected D-zone activity formed a single peak distinct from the remainder, and in addition some separation of B and C zones occurred.

Complete chromatographic separation of human placental and intestinal alkaline phosphatase was also obtained.

Immunologic evidence for serum nonspecific alkaline phosphatase heterogeneity was provided by the successive use of rabbit antihuman alkaline phosphatase and starch gel electrophoresis. Rabbit antiplacental and anti-bone human alkaline phosphatase were prepared by the subcutaneous injection of partially purified enzyme (10) in company with a complete Freund adjuvant. Individual sera with various phosphatase patterns were reacted with an antienzyme for several days. Thereafter the supernatant fluid was examined electrophoretically in parallel with a nonreacted serum sample. A, B, D, and certain F bands were uniformly removed by rabbit antiplacental phosphatase, while C-zone activity was undisturbed. Unfortunately, a certain amount of cross reaction occurred with partially purified human intestinal and liver alkaline phosphatase, and accordingly, by this technique, the identity of A, B, and D zones of activity with placental phosphatase remains in doubt. Rabbit anti-bone phosphatase removed only C-zone activity and did so completely in normal and pregnant adults, children, and a patient with Paget's disease. Rabbit anti-bone phosphatase did not cross react with partially purified alkaline phosphatases from placenta and intestine. Immunologic studies with kidney and liver alkaline phosphatase are in progress. Electrophoretically separable tissue alkaline phosphatase components can also be resolved chromatographically. Consequently the problem of cross reactions can possibly be resolved in the future by the use of antigens which are chromatographically pure. The use of anti-bone phosphatase, unlike antiplacental phosphatase, resulted in the formation of a distinct ladder of newly formed slowly migrating zones. Such zones may represent soluble antigen-antibody complexes in the states of antigen excess and antibody excess. In the case of antibody excess visible evidence is possibly provided for the existence of such complexes (11). Enzyme precipitates failed to appear in titrations between purified human bone alkaline phosphatase and its antienzyme.

Nonspecific alkaline phosphatases obtained from various human organs have been compared with serum components. Purified bone alkaline phosphatase, prepared from an osteogenic sarcoma (10), and milk alkaline phos-

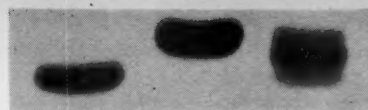


Fig. 2. Electrophoretic migration on starch gel of nonspecific alkaline phosphatase in *n*-butanol extracts of three different human placentae. The origin has been removed and only the relevant portion of analysis is shown. Migration was toward the top of page. From left to right the patterns are B, A, and AB.

phatase (12) exhibit migration similar to the C zone. Intestinal alkaline phosphatase (10), obtained from three individuals, migrated, in each case, in a fashion similar to D zone. Several F zone type components of varying mobility were also present in each organ extract. Two-dimensional electrophoresis (paper-starch gel) indicated no precise agreement of any organ alkaline phosphatase migration with any of the known major serum components. However, among 20 unselected women, the appearance of serum A and B components was in every case precisely reproduced by *n*-butanol extracts of the serum donor's placenta. Contamination of placenta with the donor's blood as a source of agreement was excluded by the failure to observe in placental extracts any of the C components present in serum. Furthermore, the phosphatase activity of placental extracts is considerable and necessitates dilution prior to electrophoretic analysis, thereby minimizing the importance of contamination by the relatively low levels of serum phosphatase. Three placental nonspecific alkaline phosphatase types in the A and B zones were observed (Fig. 2). If the unproved assumptions concerning simple genetic control of A and B zones are correct, then by a co-dominant hypothesis there should be three phenotypes,

Table 1. Proportion of women in the last trimester of pregnancy with serum alkaline phosphatase A-zone activity. Test for homogeneity of A-zone proportion among ethnic groups:  $\chi^2 = 28.9$ ; D.F. = 2;  $P < 0.001$ .

Number in group	Proportion with A-zone activity	Calculated hypothetical dominant or co-dominant A allele frequency
97	U.S. whites 0.536	0.319
100	U.S. Negroes 0.250	0.134
98	Nigerians 0.122	0.063



namely, *AA*, *AB*, and *BB*. The *AB* type possesses an *A*, a *B*, and an intense intermediate band. Sex of child had no influence on the appearance of placental phosphatase type. The relative proportions of *AA* and *AB*, which were unknown after examination of serum samples, may be projected from the hypothetical allele frequency given in Table 1, being thus approximately 1 (*AA*) : 4 (*AB*) in individuals of European ancestry. Among 12 placentae from such persons three *AA* and nine *AB* patterns appeared. The numbers involved are small; however, the observed ratio tends to support the inference of simple genetic control. Since the placenta has fetal origin, any genetically determined variation in the alkaline phosphatase of this organ would be determined by fetal genotype. The absence in umbilical cord serum of appreciable placental A and B components presumably results from a selective barrier.

It remains that a direct Mendelian proof of genetic control is unattainable at the moment. Nonetheless, if the A and B zones of serum reflect placental phosphatase heterogeneity, as seems likely, and if this diversity has a simple genetic basis, as suggested by the different proportions of A and B zones in several ethnic groups, then the differences in electrophoretic mobility of A and B zones may be due to a single amino acid substitution in placental alkaline phosphatase molecules. Alkaline phosphatase is abundant in the placenta (13), and preliminary observations suggest that considerable enzyme purification with good yield is possible. Consequently, it may be practicable to delineate further the differences between A and B placental phosphatase by peptide analysis utilizing the fingerprint technique of Ingram (14).

The multiple procedures employed in this investigation, when supplemented by examination of substrate preference, pH optimum, and response to inhibitors, may permit identification of the tissue of origin of the divers serum alkaline phosphatases observed after starch gel electrophoresis (15).

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26 May 1961

### Relative Effectiveness of Document Titles and Abstracts for Determining Relevance of Documents

**Abstract.** Individuals who received documents through a selective dissemination of information system were asked to determine the relevance of documents to their work interests on the basis of titles and of abstracts. The results indicate that there was no significant difference between the usefulness of titles and of abstracts for this purpose.

In two previous experiments, conducted by me and my associates, results seemed to indicate that titles were as useful as various forms of abstracts for determining the relevance of a specific article for a given purpose. But when titles were compared with abstracts as means of obtaining specific information on specific questions, it was found that abstracts were significantly better (1). The purpose of the experiment reported here was to compare the effectiveness of titles with that of abstracts when they were used for the purpose of notifying research workers of the availability of documents which might be relevant to their work interests.

During 1960, a selective dissemination of information system was in operation in the Advanced Systems Development Division of the International Business Machines Corporation. One of its purposes was to notify scientists, engineers, managers, and technicians in the organization of the availability of documents which might

be relevant to their work interests and to give them the opportunity to order individual copies. A general description of a system of this kind is given by H. P. Luhn (2). The system discussed here and an earlier version of it have been described in detail (3).

Briefly, the system consists of matching key words that state the interests of users with key words selected from documents processed by the system. When a predetermined percentage *p* of matches occurs, a notification is sent to the user. In the experiment under discussion this percentage was set at between 0.20 and 0.28 (for 93 percent of the documents processed, *p* was at 0.20).

The notification consists of the IBM card printed with the title of the article, the name of the author or authors, the abstract, the source of the document, and the number of pages. Upon receiving the notice, the user determines, among other things, whether or not he is interested in receiving a copy of the document (4); if he is, he responds appropriately and is sent a copy. These responses to notices are called "first responses." After reading the document, the user again responds, indicating whether or not the document was in fact relevant to his interests. These are called "second responses." From the results of previous experiments it appeared that utilizing the title only would be as effective for the purpose of ordering documents as utilizing both the title and the abstract. Two hypotheses were formulated to test this assumption.

1) There is no significant difference between the ordering rate for documents when titles are used for notification and the rate when a combination of titles and abstracts is used. ("Hard copy orders on first responses.")

Table 1. Rates of ordering and accepting documents on the basis of notification by title and by title and abstract.

First response (%)		Second response: Judgments of relevance of hard copies received (%)
Hard copy orders	Accepted notifi- cations*	
<i>Notification by title only</i>		
24.6	55.8	61.8
<i>Notification by title and abstract</i>		
24.5	58.1	58.9

\* Any of the following responses constitutes an accepted notification: (i) A hard copy is ordered; (ii) the document is of interest, though a hard copy is not ordered; (iii) the document is of interest and the user already has a copy.



2) There is no significant difference between the rate of acceptance of documents ordered on the basis of titles alone and the rate of acceptance when the document is ordered on the basis of title and abstract. ("Judgments of relevance on second responses.")

During the experiment the normal operating procedure for the system was followed, except that some of the notifications contained only the title, author or authors, source of document, and number of pages, whereas others included the abstract. Four hundred documents were selected for the experiment. These documents consisted of internal I.B.M. reports and published journal articles. Since there was no way to control the type or number of documents received by the system, it was decided to process the normal document flow as it came into the system from the various document sources. As a result, 51 percent of the documents processed were articles from the *Journal of Applied Physics*, 36 percent were internal I.B.M. reports, and 13 percent were internal I.B.M. patent disclosures.

Incoming documents are numbered consecutively in this system. Four hundred document numbers were selected and separated into two groups, on a random basis. Notifications of corresponding documents in one group contained title and abstract; notification of documents in the other group contained the title only. The documents were processed in the normal fashion, and notifications were distributed to some 400 users who were served by the system in the course of the experiment. The users were not told that an experiment was taking place. The only change from the usual procedure that they might notice was that some of the notification cards did not contain abstracts.

Results with the nonparametric Mann-Whitney U test (5) showed that both hypotheses could be accepted at the .05 level—namely, that (i) there is no significant difference in ordering rates for documents when titles are used for notification purposes and when a combination of titles and abstracts is used; (ii) there is no significant difference in the rates of acceptance of documents ordered on the basis of titles alone and of documents ordered on the basis of both title and abstract (see Table 1).

Because the documents were from only three sources, one might expect

Table 2. Rates of ordering and accepting documents, by document group, on the basis of notification by title and by title and abstract.

Document group	First response (%)		Second response: Judgments of relevance of hard copies received (%)
	Hard copy orders*	Accepted notifications†	
Notification by title			
Patents	32.8	54.2	72.6
Internal reports	30.0	60.6	67.7
<i>J. Appl. Phys.</i>	19.6	53.9	52.7
Notification by title and abstract			
Patents	33.2	54.1	67.3
Internal reports	36.2	68.2	59.9
<i>J. Appl. Phys.</i>	16.3	53.8	54.3

\* Differences between values in rows 1 and 3, 2 and 3, 4 and 6, and 5 and 6, respectively, are significant at the .05 level or below. † Differences between values in rows 2 and 3, 4 and 5, and 5 and 6, respectively, are significant at the .05 level or below.

that the source of the document had considerably biased the results of the comparison. When, however, the source of the document was considered, it was found that both hypotheses were still tenable (see Table 2). The first-response acceptance rate for the entire document population was approximately 57 percent. It was interesting to note that there was considerable variance with respect to sources of the document on both the first and the second response. For example the second-response acceptance rate for the *Journal of Applied Physics* was approximately 53 percent, whereas the second-response rate for the internal I.B.M. patent disclosures was about 70 percent and that for the internal I.B.M. reports was about 63 percent (see Table 2). In spite of these large variances, there were no significant differences in second-response acceptance rate within the title and abstract groups.

An interesting sidelight was that, despite the fact that most of the 400 users had been regularly receiving abstracts along with the titles for over 6 months, when the abstracts were omitted from 2092 notifications generated from 200 different documents, only seven users registered any complaints.

The results clearly indicate that at the percentages of match ( $p$ ) utilized in this experiment there were no significant differences, at the .05 level, between titles and abstracts as notification media for this particular system. From all indications it seems that the setting of  $p$  is one of the critical variables in determining whether a document is accepted or rejected by the user. In an earlier experiment (3), when  $p$  was set at 0.08, rather than between 0.20 and 0.28, the rate of acceptance of notifications was only

about 41 percent as opposed to about 60 percent in this experiment. Thus one can conclude that increasing the required percentage of match makes it more likely that the document will be of interest to the user, and that consequently a higher percentage of documents will be ordered. Thus, if there were a significant difference in acceptance rate, it would be less noticeable with a high  $p$ . Therefore, it seems that if significant differences are to be found they would occur when  $p$  is at some low value. This experiment shows that the value must be below 0.20. This fact has little practical utility, however, since selection of low values of  $p$  is inconsistent with the objective of effective selective dissemination of information. Selection of low values of  $p$  generally results in sending users a high proportion of unwanted notifications.

Other areas for research which could be considered are the effect of the quality of key words selected from documents for matching with users' interests. Is the educational level or professional background of the encoder a significant factor in the selection of key words, and hence in the results in a given experiment? Would a list of key words or descriptors for each document furnished the user be of more use for deciding the relevance of documents than either titles or abstracts?

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- 2 May 1961

## Cytogenic Behavior of a Knobbed Chromosome 10 in Maize

**Abstract.** Unlike maize plants heterozygous for the abnormal chromosome 10 (K10) and the normal chromosome 10 (k10), those heterozygous for a knobbed chromosome 10 (K<sub>10</sub>/k10) do not undergo preferential segregation or "meiotic drive." Ting's chromosome 10, however, does show neocentric activity, although the extent of this is not as pronounced as is the case in K10 homozygotes. The K<sub>10</sub> chromosome is considerably shorter than and is cytologically distinguishable from the K10 and the k10 chromosomes.

In the *A* genome of maize there exist several morphologically distinct forms of chromosome 10. The knobless form (k10) is the most frequently encountered one and is the shortest of the *A* chromosomes. The rare forms have been designated as "abnormal" and are longer than and uniquely different from the "normal" chromosome 10. The origin of these abnormal chromosomes 10 is still to be established, although Ting has proposed a possible mechanism (1).

The abnormal chromosome 10 designated as K10 is characterized by the presence of three small but conspicuous chromomeres in the region corresponding to the distal one-sixth segment of the long arm of the normal chromo-

somes 10 and by the presence of a foreign segment of chromatin possessing a very large heterochromatic knob in the subterminal position. Of the several forms of abnormal chromosomes 10 present in the maize genome, this K10 form is the most common (2). In the presence of this abnormal chromosome, several intriguing events occur: (i) the formation of neocentromeres in regions other than the true centromeres (3, 4); (ii) the precocious anaphase movement of chromosomes possessing such newly arisen centromeres during meiosis with the neocentric regions leading the chromosomes to the poles (3); (iii) the preferential segregation or nonrandom recovery of certain chromosomes in the egg cells during megasporogenesis (4, 5); and (iv) an increase in the frequency of chiasma formation (6) and crossing over (7, 8) in the chromosomes thus far studied. The neocentric activity is especially pronounced in plants homozygous for the K10 chromosome, and it is not uncommon to observe highly attenuated chromosomes at meiotic anaphases.

A second form of abnormal chromosome 10, here designated as Ting's abnormal chromosome 10 (K<sub>10</sub>), is readily distinguishable from the K10 form. The K<sub>10</sub> chromosome is longer than the normal chromosome 10 by approximately one-fifth the length of the latter. In the subterminal position of this abnormal chromosome is located a large knob, and in the region corresponding to the distal one-sixth of the long arm of the normal chromosome 10 is found a single chromomere which is very often quite conspicuous. Ting found this particular form of chromosome 10 in one of three plants from a cross between a Peruvian variety and a U.S. inbred strain. The plant possessing the K<sub>10</sub> chromosome carried no supernumerary or *B*-chromosome, while each of the other two sister plants was homozygous for the normal chromosome 10 and possessed a single *B*-

chromosome. According to Ting, the foreign piece of the K<sub>10</sub> chromosome closely resembles the proximal region of the *B*-chromosome which includes the terminal centromere and the large, elongate, pycnotic region (1). He also reported the existence of a "high degree of 'homology'" between the knob of the K<sub>10</sub> chromosome and the pycnotic regions of the *B*-chromosome (9). It is of interest to note that Rhoades and Dempsey found no appreciable frequency of association between the knob of the K10 chromosome and the *B*-chromosome (10). Because of the frequent association of the pycnotic regions of the K<sub>10</sub> chromosome and the *B*-chromosome and the apparent morphological similarity between the foreign segment of the K<sub>10</sub> chromosome and the proximal region of the *B*-chromosome, Ting has suggested that the K<sub>10</sub> chromosome arose as a consequence of a simple translocation involving the normal chromosome 10 and the *B*-chromosome (1). As is the case with the K10 chromosome, neocentric activity was observed during the two meiotic divisions of microsporogenesis in plants carrying the K<sub>10</sub> chromosome (1).

During the course of the present study it was found that Ting's abnormal chromosome 10 is considerably shorter than the K10 chromosome, the end of the long arm approximately coinciding with the position occupied by the most distal of the three conspicuous chromomeres of the K10 chromosome. Furthermore, the knob present on K<sub>10</sub> is only about a third as large as that found on the K10 chromosome. Insofar as neocentric activity is concerned, plants homozygous for the K<sub>10</sub> chromosome display activity not unlike that found in K10/k10 heterozygotes.

Inasmuch as both forms of abnormal chromosome 10 possess neocentric activity and since the preferential segregation hypothesis (3, 4) involves the action of these neocentromeres, it was deemed of interest to ascertain whether Ting's abnormal chromosome 10 is also capable of inducing preferential segregation during megasporogenesis. The data presented below represent the results of the initial experiment.

In this experiment the normal or k10 chromosome was genetically marked with the dominant *R* (aleurone color) factor, whereas the abnormal partner, K<sub>10</sub>, was marked with the recessive *r* (no aleurone color) allele. At the same time, one of the chromosomes 9 which

Table 1. Results from backcrosses of *R* k10 / *r* K<sub>10</sub>; K<sup>M</sup>9 *Sh* / K<sup>B</sup>9 *sh* compounds.

Family	Kernels (No.)						$\chi^2(1:1)$	<i>P</i>
	<i>R</i>	<i>r</i>	Total	<i>Sh</i>	<i>sh</i>	Total		
61:35	1366	1332	2698				0.428	>.50
61:36	1388	1380	2178				0.649	>.30
Totals	2704	2712	5416*				0.012	>.90
61:35				975	956	1931	0.042	>.80
61:36				1053	1049	2102	0.0076	>.90
Totals				2028	2005	4033*	0.132	>.70

\* The two populations are not of identical size because a few of the plants were pollinated by *r* testers homozygous for the *Sh* factor.

possessed a medium-sized knob ( $K^M$ ) was marked with the dominant *Sh* (plump kernel) factor, and the homolog possessing a small-sized knob ( $K^S$ ) was genetically marked with the recessive *sh* (shrunken kernel) allele. Plants with the above cytogenetic constitution were employed as female plants in backcrosses to plants homozygous for both the *r* and *sh* alleles. It should be stated that preferential segregation is not confined to chromosome 10. Preferential segregation of other chromosomes occurs in  $K10/k10$  heterozygotes when the two members of a pair differ by one possessing a knob while the other is knobless or if the two homologs have knobs of dissimilar size. If the  $K_{\tau}10$  chromosome were as efficient as the  $K10$  form in inducing preferential segregation, one would expect to find approximately 70 percent of the backcross kernels to be colorless (3, 4, 7) and about 60 percent to be "plump" in phenotype (7). The data presented in Table 1 clearly demonstrate the inability of the  $K_{\tau}10$  chromosome to effect preferential segregation. Thus one would not expect to observe "meiotic drive" (11) in a population in which only the normal and the  $K_{\tau}10$  chromosomes were segregating. Rather, these two forms of chromosome 10 should exist in stable proportions. On the other hand, a population containing equal numbers of the  $K10$  and  $K_{\tau}10$  forms should in time convert into a population in which the  $K10$  chromosome is more prevalent.

Whether the absence of preferential segregation is related to the low level of neocentric activity is not known. Emmerling has noted that two modified forms of the  $K10$  chromosome elicit a low level of neocentric activity and has suggested that this reduced neocentric activity could be responsible for the random segregation ratios she has observed in her experiments involving these modified chromosomes (12). A similar situation may very well exist here also (13).

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2 June 1961

### Metabolic Block in Utilization of Galactose by Strain L Tissue Culture Cells

**Abstract.** Evidence is presented which suggests that the utilization of galactose by mouse strain L-cells is blocked by the absence of epimerase activity in this strain.

Abnormalities in the metabolism of galactose occur in a variety of organisms. The precise nature of these abnormalities has been studied in humans and, more recently, in bacteria. Thus Schwarz *et al.* (1) showed that when erythrocytes of carriers of the heritable defect galactosemia were incubated with galactose, galactose-1-phosphate (Gal-1-P) accumulated in the erythrocytes. Kalckar and his collaborators (2) demonstrated that this accumulation of Gal-1-P was caused by the lack of Gal-1-P uridyl transferase (transferase) activity in galactosemics. Different mutants of the bacterium *Escherichia coli* unable to utilize galactose as a source of carbon have been shown to be deficient in galactokinase (3), transferase (3), and uridine diphosphogalactose-4-epimerase (epimerase) (4) activity, respectively. Triply defective mutants have also been found (4).

Eagle *et al.* (5) have reported that galactose does not support the growth of L-cells, a strain of mouse fibroblasts (6). We confirmed this observation and also found that L-cells do not respire galactose. In this report it is shown that the inability of L-cells to utilize galactose as a source of carbon may be explained by the absence of epimerase activity in these cells.

L-cells were grown as described earlier (7); lactalbumin hydrolyzate medium (8) was substituted for tissue culture medium 199. The determination of the presence of the enzymes required for the metabolism of galactose was based in part on the identification of intracel-

lular galactose and in part on enzyme assays. L-cells which had been incubated for 180 minutes at 37°C in lactalbumin hydrolyzate medium containing 100  $\mu$ mole  $1-C^{14}$  labeled galactose per milliliter were harvested by sedimentation, washed, and boiled in a small amount of water; and the cellular debris was removed by centrifugation. The supernatant was then freed of protein by ultrafiltration through a dialysis membrane and chromatographed with the use of Leloir's ethanol-acetate solvent (9). A portion of this protein-free extract was retained for the enzymic determination of Gal-1-P which had been formed (see below). The ethanol-acetate solvent provided excellent separation of the three compounds, galactose, Gal-1-P, and uridine diphosphogalactose (UDPGal). Thus, the  $R_f$  value of galactose is about 0.65, Gal-1-P has a  $R_f$  value of about 0.32, and uridine diphosphoglucose (UDPG) of 0.18. Since UDPG and UDPGal have the same  $R_f$  value in this solvent (9), UDPG was used as the reference standard for UDPGal.

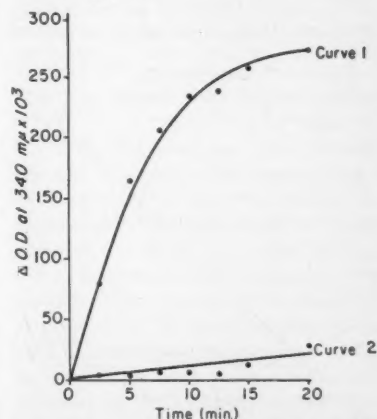


Fig. 1. Demonstration of Gal-1-P in L-cell extracts. Cuvette 1 (curve 1) contained the following assay mixture: 0.5 ml of buffer (0.006M histidine-HCl, pH 7.5); 1  $\mu$ mole of  $MgSO_4$ ; 0.6  $\mu$ mole of TPN; 0.5  $\mu$ mole of UDPG; yeast enzyme preparation, 0.05 ml; 0.2 ml of ultrafiltrate (see text) prepared from L-cells incubated in the presence of 0.005M galactose. Cuvette 2 (curve 2) contained 0.2 ml of boiled extract prepared from L-cells incubated in the absence of galactose. Cuvette 3 (control for endogenous reduction of TPN by yeast extract) contained 0.4  $\mu$ mole of added Gal-1-P and no L-cell extract, and cuvette 4, which contained the yeast enzyme preparation (0.05 ml) and TPN, served as blank. The total volume in all cuvettes was 1 ml. The course of the reaction was followed by measuring TPN reduction at 340 m $\mu$ .



The radioactive material tentatively identified as UDPGal ( $R_f$ , 0.18) was eluted from the paper in 1N HCl, boiled 10 minutes, and rechromatographed in an isopropanol-water (4:1) solvent (10) which separates galactose from glucose ( $R_{\text{glucose}}$  of galactose, 0.80). Over 70 percent of the radioactivity was then associated with the galactose area.

In the L-cell extract prepared according to the above procedures, 88 percent of the radioactivity was found to be associated with galactose, 8 percent with Gal-1-P, and the remaining 4 percent migrated with a  $R_f$  characteristic of UDPGal. As much as 6  $\mu\text{mole}$  Gal-1-P and 3  $\mu\text{mole}$  UDPGal accumulated per milliliter of packed cells.

When L-cells were incubated in the presence of lower concentrations of galactose in the medium (5  $\mu\text{mole}/\text{ml}$ ), approximately 10 percent of the total intracellular radioactivity was associated with UDPGal (0.59  $\mu\text{mole}/\text{ml}$  of packed cells). Appropriate experiments indicated that less than 0.05  $\mu\text{mole}$  of UDPG was present per milliliter of packed cells. In the presence of still lower concentrations of galactose in the medium (0.05  $\mu\text{mole}/\text{ml}$ ), 54 percent

of the total intracellular radioactivity was associated with UDPGal (0.42  $\mu\text{mole}/\text{ml}$  of cells).

The presence of 28  $\mu\text{mole}$  of galactose per milliliter of medium did not significantly inhibit the growth of L-cells when glucose served as a source of carbon. A slight increase in the lag period prior to the onset of exponential growth was observed.

The presence of Gal-1-P in extracts from L-cells incubated with galactose was also demonstrated by the reduction of triphosphopyridine nucleotide (TPN) (2) in the presence of an extract from yeasts adapted to galactose (Fig. 1). Attempts to measure galactokinase activity in crude L-cell extracts by the method of Horecker *et al.* (11) were unsuccessful. Transferase activity was determined by the method of Kurahashi and Anderson (12). Instead of the glycine buffer employed by these authors a histidine-HCl (0.006M) buffer at pH 7.5 was used. L-cell extracts prepared by freeze-thawing were found to be rich in phosphoglucomutase and glucose-6-phosphate dehydrogenase activity, and hence it was not necessary to add these enzymes to the assay preparation. Since UDPGal was not available to us, we could not measure epimerase activity directly by the method of Kalckar *et al.* (4). However, by limiting the amount of UDPG in the assay mixture to catalytic, rather than substrate, concentrations, the relative activity of epimerase could be measured: in the absence of epimerase, TPN reduction is dependent upon the concentration of UDPG, whereas in the presence of epimerase, TPN reduction requires only catalytic amounts of UDPG.

Comparison of curves 1 and 3 (Fig. 2) shows that the L-cell extracts had transferase activity (0.10  $\mu\text{mole}$  UDPGal formed in 2 hours) but no detectable epimerase activity. The effectiveness of the assay procedure was demonstrated by the finding that, under identical assay conditions, freshly excised mouse liver displayed epimerase as well as transferase activity.

The results reported here suggest strongly that the inability of L-cells to use galactose as a source of carbon reflects the absence of epimerase activity in this cell strain (13).

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## Relationship of Blood Type to Histocompatibility in Chickens

**Abstract.** Evidence shows that the B blood group locus in chickens, which controls red cell antigens, is associated with tolerance of skin homografts. Three other blood group loci studied did not show this effect.

The various factors involved in transplantation biology have been studied by workers in several scientific areas. Investigations have been most fruitful in those species where inbred lines are available.

In the case of chickens, tolerance to skin homografts is associated with degree of kinship between donor and host (1) as well as with age at grafting (2). Early workers, using heteroimmune hemagglutinins, reported that homograft tolerance and blood type were independent (3). Experiments involving induced homograft tolerance by embryonic transfusion of whole blood or its separate components have led to contradictory conclusions. Red blood cells were held to be ineffective in inducing tolerance in one case (4) but were found to be the most effective cell fraction in another (5). High incidence of homograft acceptance has been observed in one inbred line of chickens known to be segregating at the A blood group locus (6).

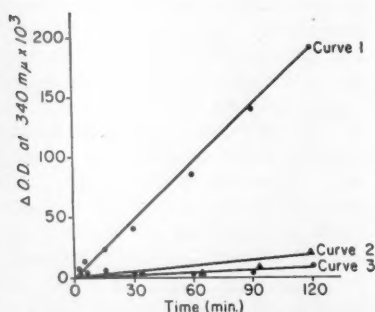


Fig. 2. Demonstration of transferase activity in L-cell extracts. The composition of the assay mixtures was similar to that used in the assay for Gal-1-P (Fig. 1). However, 0.01  $\mu\text{mole}$  of diphosphopyridine nucleotide (14) was added to all reaction vessels in addition to the TPN; the yeast enzyme preparation was omitted. L-cell extracts were prepared by freeze-thawing and contained 8 mg of protein per milliliter: 0.05 ml of this extract was added to the cuvettes, and the total volume in all cuvettes brought to 1 ml with  $\text{H}_2\text{O}$ . Curve 1, L-cell extract incubated in the presence of 0.1  $\mu\text{mole}$  of Gal-1-P. Curve 2, L-cell extract incubated in the presence of 0.4  $\mu\text{mole}$  of Gal-1-P only. Curve 3, extract incubated in the presence of 0.01  $\mu\text{mole}$  of UDPG and 0.4  $\mu\text{mole}$  of Gal-1-P. No TPN reduction occurred when extracts were incubated with 0.1  $\mu\text{mole}$  of UDPG only.

Table 1. Influence of *B* locus genotype with respect to homograft tolerance.

Family designation No.	Birds (No.)	Genotype of host	No. of grafts accepted (+) and rejected (-) from donor of genotype shown					
			<i>B</i> <sup>1</sup> / <i>B</i> <sup>1</sup>		<i>B</i> <sup>1</sup> / <i>B</i> <sup>2</sup>		<i>B</i> <sup>2</sup> / <i>B</i> <sup>2</sup>	
			(+)	(-)	(+)	(-)	(+)	(-)
3168	3	<i>B</i> <sup>1</sup> / <i>B</i> <sup>1</sup>	4	0	0	8		
	4	<i>B</i> <sup>1</sup> / <i>B</i> <sup>2</sup>	8	0	6	0		
3159	4	<i>B</i> <sup>1</sup> / <i>B</i> <sup>1</sup>	7	0	0	6		
	3	<i>B</i> <sup>1</sup> / <i>B</i> <sup>2</sup>	6	0	6	0		
19	2	<i>B</i> <sup>1</sup> / <i>B</i> <sup>2</sup>					6	0
	3	<i>B</i> <sup>2</sup> / <i>B</i> <sup>2</sup>			0	6	5	0

The purpose of the experiment reported here (7) was to determine genetic relationships between different blood group genotypes and histocompatibility in an inbred line of Leghorn chickens. Nineteen chicks from three females but having a common sire were used. Inbreeding coefficients ranged from 0.45 to 0.49. Two alleles were segregating at each of four independent loci determining red cell antigens. This included the *A*, *B*, and *L* loci in family No. 3159, the *B*, *D*, and *L* loci in family No. 3168, and the *B* and *L* loci in family No. 19. Prior to grafting, the chicks were blood typed by tube agglutination with isoimmune sera. These sera, considered specific for this line of birds, were used to identify antigens designated *A*<sub>1</sub>, *A*<sub>2</sub>, *B*<sub>1</sub>, *B*<sub>2</sub>, *D*<sub>1</sub>, *D*<sub>2</sub>, *L*<sub>1</sub>, and *L*<sub>2</sub>. Reference reagents, supplied by a commercial firm, were used to establish that the blood group systems studied corresponded to the systems ascertained by other workers (8).

A total of 70 whole thickness skin grafts were made 16 days after hatching occurred. Tissue was exchanged between full sibs only and also reciprocally so that each chick was both donor and host. Chicks received from two to four 8-mm-square grafts, but not more than one graft from the same donor. Location of the graft was randomly assigned to one of four positions on the back. Grafts were exchanged such that the donor had 0, 1, or 2 identifiable red cell antigens not in common with the host. Beginning on the 6th day after grafting began, grafts were observed daily for 1 week and on alternate days thereafter. The grafts were scored for degree of vigor by a series of arbitrary grades (2) which provided an accurate determination of graft rejection time.

Two of the homografts were lost because of faulty technique soon after grafting. In 38 of the remaining 68 grafts, the donor possessed one or more red cell antigens not possessed by the

host. Twenty such donor-host red cell incompatibilities were, with respect to *B* antigens, either alone or in combination with antigens of the other three systems. These 20 grafts exhibited the typical pattern of an immunological reaction by showing signs of rejection on the 7th postoperative day; all had been sloughed off 5 days later. The remaining 48 grafts appeared healthy and were considered to have been accepted by the host at this time.

Thus, rejection occurred only when *B*<sub>1</sub> or *B*<sub>2</sub> donor red cell antigens were not also present in the host. The results presented in Table 1 indicate that the *B* locus in chickens not only determines red cell antigens but is also a major histocompatibility locus. This would suggest that red cells may share antigens with the skin which are directly involved in histocompatibility. Studies with birds of known blood type should help to explicate the contradictory results obtained where erythrocytes have been used to induce skin graft tolerance.

The number of remaining donor-host differences involving loci *A*, *D*, and *L* were 8, 4, and 13, respectively. These differences, as well as differences in sex, were not found to influence histocompatibility to the 40th postoperative day. However, five grafts were rejected during the 5th week after grafting. A gradual and less violent reaction was observed in these cases than that previously noted. Thus, it appears that alleles at other loci determining comparatively weak histocompatibility antigens may be segregating in this inbred line. It is well known that sloughing often occurs even after long periods of graft tolerance. Alleles and loci other than those examined in this study may well have variable differences in their effects on histocompatibility.

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4 May 1961

### Electrophoretic Analysis of the Serum Proteins of Neurological Mutations in Mice

**Abstract.** The serum proteins of three neurological mutations, tremulous, quivering, and waddler, were studied. The albumin level rose while the globulin level, especially the level of alpha fraction, fell in tremulous mice, but the trend was reversed in quivering mice. In waddler mice the changes were observed only in females.

During the past few years a large number of neurological mutations have been reported in mice by various investigators. These mutations, as briefly reviewed by Yoon (1), may be classified into two large categories: the waltzer-shaker type and the trembler-waddler type. A rapid circular movement is a common characteristic of the waltzer-shaker type, but this movement is absent in the trembler-waddler type. Instead, the latter shows various combinations of the following traits: tremor, paralysis of front or hind legs or of both, muscular incoordination, loss of straightening reflex, locking hindlegs when picked up by the tail, priapism, epileptic form of convulsion, and reduction or loss of fertility. In the search for a biochemical basis for these genetic abnormalities, the serum protein patterns of three trembler-waddler type mutations, quivering (gene symbol, *qv*), waddler (gene symbol, *wd*), and tremulous (gene symbol, *tm*) (2) were studied. Electrophoresis was carried out in a Durrum-type electrophoresis cell with Schleicher and Schuell 2043-A mgf paper strips (Spinco No. 300-846) and

Table 1. Relative levels of serum proteins, in percentages, in three strains of mice, Tm, Qv, and Wd. All three mutations are simple recessives. Therefore, Tm<sup>-</sup>, Qv<sup>-</sup>, and Wd<sup>-</sup> indicate normal mice from the Tm, Qv, and Wd strains, and tmtm, qvqv, and wdwd indicate affected mice from the respective strains.

Strains	Genotypes	Sex	No. of determinations	Albumin			Globulin		
				$\bar{x}$	$s\bar{x}$	Difference, $P$	Alpha	Beta	Gamma
Tm	Tm <sup>-</sup>	M, F	28	66.95	0.59	<0.001	10.59	14.96	7.50
	tmtm	M, F	14	74.76	1.17		6.22	12.69	6.33
Qv	Qv <sup>-</sup>	M, F	10	66.36	0.80	<0.05	11.18	14.44	8.02
	qvqv	M, F	10	60.55	1.97		11.46	17.17	10.82
Wd	Wd <sup>-</sup>	M, F	14	70.45	0.52	<0.05	9.00	12.89	7.66
	wdwd	F	8	73.95	1.49		5.89	11.34	8.82
	wdwd	M	8	70.81	0.76		10.25	11.73	7.21

a constant current of 2.5 ma (75 volts) for 16 hours, by use of the improved techniques described by William, Pickels, and Durum (3). A barbiturate buffer, pH 8.6, ionic strength 0.075, was used as electrolyte. The elution technique was adopted for the quantitative determination of the proteins, and a spectrophotometer at a wavelength of 590 m $\mu$  was used to read the solution. Bromophenol blue was the dye employed in this procedure. The serum components were labeled as albumin and as alpha, beta, and gamma globulin, in analogy with components of human serum. No effort was made to identify different fractions of each of these components. Both affected and normal mice between 5 and 7 months of age were obtained from strains designated as Quivering (strain symbol, Qv), Waddler (strain symbol, Wd), and Tremulous (strain symbol, Tm) that have been maintained by sib matings for various numbers of generations at Genetics Laboratory of Boston College.

The results of this study are summarized in Table 1. The relative levels of the various components of the serum proteins vary only slightly from strain

to strain among normal mice. However, significant changes were observed in the affected animals of all three strains. In the Tm strain, a significant proportional increase in the albumin level and accordingly a proportional decrease of the globulin level were observed. Most of the decrease in the globulin level was accounted for by the sharp decrease in alpha fraction. In the Qv strain, in contrast to the Tm strain, the level of albumin fell considerably in the affected mice and there was an increase in the globulin level. Increased levels were seen in both the beta and gamma fractions, but the alpha fraction remained practically the same as in the normal mice. This is graphically shown in Fig. 1. These differential changes of the serum protein patterns seem to indicate that, although tremulous and quivering mice are strikingly similar in their phenotypic expression, they may result from alterations in different biochemical pathways.

In the Wd strain, the result was more complicated. The affected males showed no significant changes as compared to their normal sibs, but the affected females showed tendencies similar to

those observed in the Tm strain. The level of albumin rose and that of the globulin, especially the alpha fraction, dropped. This indicates that the changes in waddler females may be secondary rather than primary effects of this genetic disorder. Since it was observed that the variances in affected mice of all three strains were higher, although statistically not significant, than those in the normal mice, this may be true for tremulous and quivering mice also, if it is assumed that the higher variances are due to gradual changes that take place in the affected mice and that some mice are affected more severely than others at any particular time (4).

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11 July 1961

#### Preliminary Method for Estimating Stability in Plankton

**Abstract.** The stability of plankton in the York River, Virginia, during the summer of 1960 is computed by a derived empirical stability measure. The communities were indicated to have considerable homeostasis, being over five times more stable than the physical biotope.

In connection with a study of plankton productivity in the York River during the summer of 1960, data were obtained at a station about 300 yards off the pier at the Virginia Institute of Marine Science in 10 consecutive weeks (between 22 June and 25 August) for a number of pertinent variables. In studying these data, it became desirable to know something about the resistance of the communities to change of state.

A number of stability tests are available for linear systems from formal stability theory (1), but unfortunately these depend mostly on the nature of the latent roots  $\lambda$  (eigenvalues) of the characteristic equation.

$$\phi(\lambda) = \lambda^n + m_1 \lambda^{n-1} + m_2 \lambda^{n-2} + \dots + m_n = 0 \quad (1)$$

of matrix  $A$  in the homogeneous equations

$$(A - \lambda I)X = 0 \quad (2)$$

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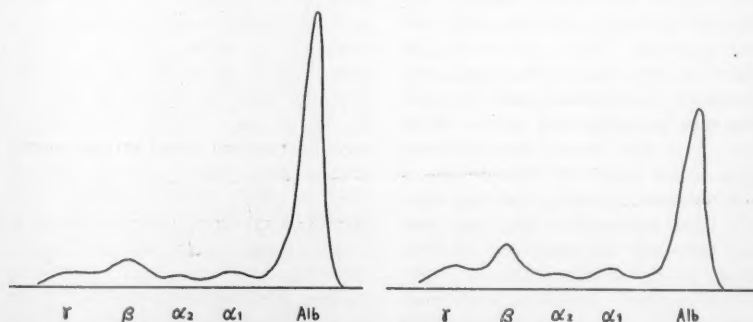


Fig. 1. Electrophoretic patterns of tremulous (left) and quivering (right) mice, showing changes in opposite directions.



where  $A$  is the coefficient matrix of the canonical representation of the system shown in Eq. 3:

$$\begin{aligned} dx_1/dt &= a_{11}x_1 + a_{12}x_2 + \dots + a_{1n}x_n + b_1 = 0 \\ dx_2/dt &= a_{21}x_1 + a_{22}x_2 + \dots + a_{2n}x_n + b_2 = 0 \\ &\vdots \\ dx_n/dt &= a_{n1}x_1 + a_{n2}x_2 + \dots + a_{nn}x_n + b_n = 0 \end{aligned} \quad (3)$$

and  $X$  is a column vector whose components are the variables  $x_1, x_2, \dots, x_n$ . Since it is usually not possible to obtain sufficient data to represent natural ecosystems canonically and since they are probably not linear, formal stability criteria are not generally available for ecological applications. The only realistic approach is to develop empirical measures. MacArthur (2) has provided such a measure for food webs, relating their stability to their entropy. A stability criterion for  $n$  variable ecosystems is formulated below and applied to the variables of the York River listed in Table 1. The primary data are fully reported elsewhere (3) with details of the methods employed.

In considering the stability of plankton, two basic problems are (i) how

stable are they and (ii) are they more stable than their immediate environment? If all the variables of an ecological system were random variables, randomly sampled, then each variable might be regarded as most stable if and when the probability for an increase in value when low and for a decrease when high were unity. Assuming the stability of the whole system to be in direct relation to that of the individual variables (not a sound assumption, technically, but acceptable for first approximation purposes), then a simple stability measure might be

$$\sigma = \frac{1}{n} \sum_{j=1}^n s_j \quad (4)$$

where  $\sigma$  is the stability of the system, and  $s_j$  that of the  $j$ th of  $n$  variables. For stability of the separate variables, consider the matrix of transition probabilities  $P$ :

$$P = \begin{bmatrix} p_{11} & p_{12} \\ p_{21} & p_{22} \end{bmatrix} \quad (5)$$

where  $p_{12}$  represents the probability for a decrease following an increase,  $p_{11}$  that for an increase following an increase, and so forth. We may associate with  $P$  a determinant, and employ it as a stability measure for the  $j$ th

variable. Thus, Eq. 4 can be written

$$\sigma = \frac{1}{n} \sum_{j=1}^n \det P_j \quad (6)$$

where  $\sigma$  has a range  $-1 \leq \sigma \leq 1$ . Null stability of the system is denoted by  $\sigma = 0$ , instability by  $\sigma < 0$ , and stability by  $\sigma > 0$ .

By separating all the variables of the York River for which data were obtained into two groups, physical and biological stabilities can be computed separately for the environment ( $\sigma_E$ ) and for the plankton ( $\sigma_P$ ). The calculations are summarized in Table 1. The results,  $\sigma_E = 0.12$  and  $\sigma_P = 0.65$ , indicate that the plankton were 5.4 times more stable than the physical habitat. This degree of homeostasis (0.65 on a scale with maximum value unity) seems quite remarkable considering how loosely integrated plankton communities are usually thought to be (4).

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15 May 1961

#### Bow Wave Riding of Dolphins

**Abstract.** Dolphins riding bow waves were observed through underwater viewing ports. The postures assumed by the dolphins show that they present their bodies rather than the ventral surfaces of their flukes to the propellant forces of the bow wave.

How dolphins ride bow waves is the subject of a current controversy. After observing them performing this feat through underwater viewing ports in a ship's bow, I would like to report the method the dolphins themselves seem to consider proper.

Since Woodcock (1) noticed the "motionless" swimming of dolphins and interpreted it as bow wave riding, several proposals attempting to reconcile it with hydrodynamic theory have been advanced. Woodcock and McBride (2) concluded that only under conditions of laminar flow and nonbuoyancy could a dolphin sufficiently overcome drag to get a ride. Hayes (3) then showed in

Table 1. Stability determinations for plankton in the York River during the summer of 1960. "Trajectory" refers to the line of behavior of each variable (averaged for the whole water column) in terms of whether it increased (i) or decreased (d) over the preceding observation.

Variables (j)	Trajectory	$P_{id}$	$P_{di}$	$P_{ii}$	$P_{dd}$	$\det P_j$
<b>Environment</b>						
Temperature	iiididdd	.60	.67	.40	.33	.27
Chlorinity	iiiidddd	.20	.33	.80	.67	-.47
Dissolved oxygen	iddiiddid	.75	.50	.25	.50	.25
Nitrate-nitrogen	diidddid	.67	.50	.33	.50	.17
Inorganic phosphorus, dissolved	diidddiidi	.50	.75	.50	.25	.25
Inorganic phosphorus, adsorbed	iddiiddid	.40	.33	.60	.67	-.27
Organic phosphorus, dissolved	iddiiddidd	.67	.20	.33	.80	-.13
Total solids	ididdiidd	.75	.75	.25	.25	.50
Extinction coefficient	ididdiidd	.75	.50	.25	.50	.25
Secchi disc	didiiddii	.50	.67	.50	.33	.17
Light intensity	dddiddidd	1.00	.33	.00	.67	.33
$\Sigma \det P_j$						1.32
$\sigma_E = 1/11 \Sigma \det P_j$						0.12
<b>Plankton</b>						
Total chlorophyll	diididdid	1.00	1.00	.00	.00	1.00
Chlorophyll a*	ididdiidd	.60	1.00	.40	.00	.60
Chlorophyll b*	didiiddid	.75	1.00	.25	.00	.75
Chlorophyll c*	(none present)					
Carotenoids, astacin*	ididdiidd	.75	1.00	.25	.00	.75
Carotenoids, non-astacin*	(none present)					
Organic phosphorus, particulate	ididdiidd	.60	.67	.40	.37	.25
Biomass	ididdiidd	1.00	.60	.00	.40	.60
Cell counts	ididdiidd	.80	1.00	.20	.00	.80
Number of species	diididdid	.75	1.00	.25	.00	.75
Gross production	ididdiidd	.80	1.00	.20	.00	.80
Respiration	ididdiidd	.60	.67	.40	.33	.25
Net production	didiiddid	.60	1.00	.40	.00	.60
$\Sigma \det P_j$						7.15
$\sigma_P = 1/11 \Sigma \det P_j$						0.65

\* Information provided by M. L. Brehmer.

a terse presentation of calculations of hydrodynamic forces that the buoyancy of a dolphin in a pressure gradient determined the amount of hydrodynamic lift needed but did not affect the drag. Approaching the problem experimentally, Scholander (4) concluded that the tail flukes of a dolphin in the incline of a bow wave and properly angled up into the incline would be subject to enough force to push the whole animal forward. Objections by Hayes (5) to Scholander's hypothesis and a rebuttal by Scholander (6) were printed in *Science*. A point in disagreement was whether the forward component of the bow wave forces acted on the entire dolphin or just on its flukes. Fejer and Backus (7), stating that Hayes's explanation was correct but incomplete, supplied more observations on porpoises and computations on the nature of the pressure field and the function of ship's length and drag coefficient on the position of a wave-riding porpoise.

Underwater observation ports installed in the bow of the *Charles H. Gilbert* (Fig. 1)—research ship of the Bureau of Commercial Fisheries Biological Laboratory, Honolulu, Hawaii—in early 1960 for the purpose of studying tuna behavior provided excellent facilities for observing and photographing dolphins riding the bow wave. Since that time I have had occasion to observe several species of dolphins off the coast

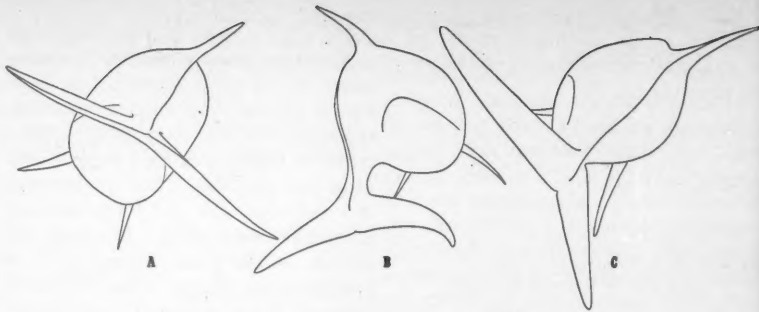


Fig. 2. Postures of *Tursiops* sp. while riding a bow wave.

of northern California, off the coast of Mexico, and near Kingman Reef in the central equatorial Pacific. Motion pictures were successfully taken at the last location.

Porpoises did not always find the bow wave suitable for riding. When they first approached, they swam from side to side in front of the ship for a few minutes, apparently testing the pressure field, before they engaged in wave riding or left.

The field observations and reviews of the movie show that the dolphins assume several attitudes. The range of postures they assumed while riding the bow wave of the ship, which traveled at about 9 knots is shown in the outline drawings taken directly from the film (Fig. 2). The posture shown in Fig. 2B is the one most commonly

seen. The trailing edge of the flukes was an estimated  $\frac{1}{2}$  to 3 feet ahead of bow. The position and predominant posture of the dolphins indicate that their bodies are used to take advantage of the forward component of the forces of the bow wave. Moreover, they were never seen riding with their flukes angled in conformance with Scholander's hypothesis. The flukes seemed to be used solely to control hydrodynamic lift.

Although the dolphins often rode steadily, without changing their positions relative to the bow wave, they seem to prefer weaving laterally in and out among each other. In doing so they continually changed their vertical positions by as much as 3 feet and thus exhibited excellent control of hydrodynamic lift and stability by changing the planes of their pectorals and flukes, bending their bodies, and banking. Implied in Hayes's explanation is the necessity of lift control to enable the dolphins to meet exigencies caused by buoyancy changes. Therefore, although such control was postulated (6, 8) to support Scholander's hypothesis, this assumption also supports Hayes's explanation (9).

HEENY S. H. YUEN

U.S. Department of the Interior,  
Bureau of Commercial Fisheries  
Biological Laboratory,  
Honolulu, Hawaii

#### References and Notes

1. A. H. Woodcock, *Nature* 161, 602 (1948).
2. —, and A. F. McBride, *J. Exptl. Biol.* 28, 215 (1951).
3. W. D. Hayes, *Nature* 172, 1060 (1953).
4. P. F. Scholander, *Science* 129, 1085 (1959).
5. W. D. Hayes, *ibid.* 130, 1657 (1959).
6. P. F. Scholander, *ibid.* 130, 1658 (1959).
7. A. A. Fejer and R. H. Backus, *Nature* 188, 700 (1960).
8. M. S. Gordon, *Science* 133, 204 (1961).
9. I wish to thank R. A. Barkley for clarifying some points in hydrodynamics. This report is published by permission of the director, U.S. Bureau of Commercial Fisheries.

1 May 1961

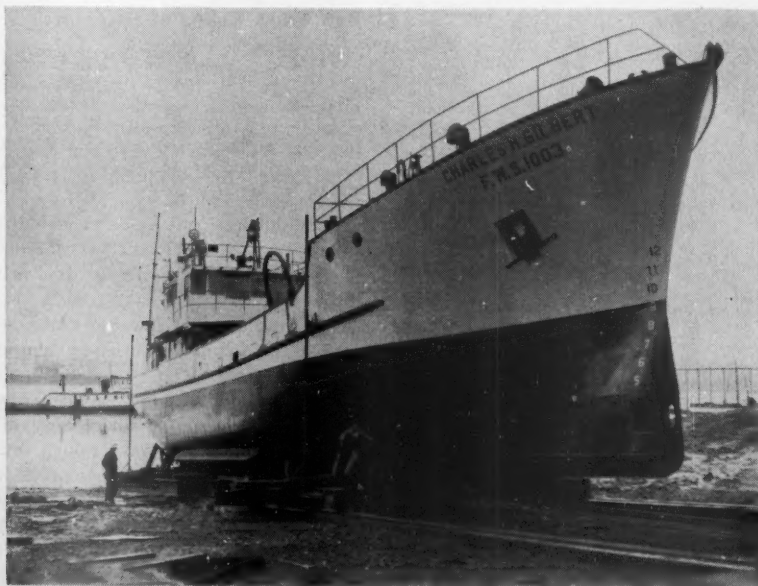


Fig. 1. Underwater viewing ports in the bow of the *Charles H. Gilbert*.

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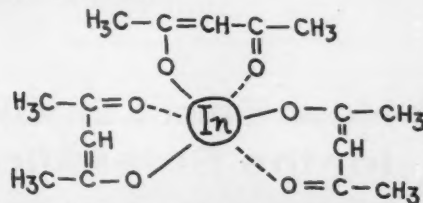
another sect can't see for dirt. One sect prefers automatic oscillogram processors. Paper manufacturers like us find their favor worth competing for. Therefore we announce a new advance in media for their use.

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"THIS" won't do for a trademark. (The code name for the field trials was "Kind 1534.") Let's call it Kodak Ektaline Paper. It comes in the two usual speeds for oscillographs, Kodak Ektaline 16 Paper and Kodak Ektaline 18 Paper. Kodak Ektaline Chemicals come as liquids. The stabilization principle used in the automatic oscillogram processors came from Kodak, too. An inquiry to Eastman Kodak Company, Photorecording Methods Division, Rochester 4, N. Y., puts everything in place right up to the moment.

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# Meetings

## Limnology and Oceanography

Symposia on two subjects of highly scientific and social significance were prominent in the two-and-a-half-day program of the Pacific Section of the American Society of Limnology and Oceanography (ASLO), during the 42nd annual meeting of the Pacific Division of the AAAS, at Davis, California. Earlier in the meeting the wide scope of scientific disciplines involved in studies of the aquatic world was illustrated in sessions for contributed papers. Topics ranged from physical and chemical conditions in a California lake, through observations of temperature variations and fish behavior from a fixed tower in the ocean, to laboratory studies of the respiration of individual sardine eggs and larvae.

The first symposium, on the scientist's responsibility in the nuclear age, jointly sponsored by ASLO, the AAAS Committee on Science in the Promotion of Human Welfare, and the Scientists'

Committee for Information on Radiation (San Francisco area), was chaired by George E. Pake, of Stanford University. The participants were Robert B. Brode and Arthur Rosenfeld (University of California), Leonard A. Herzenberg (Stanford University), Lester Breslau (California Department of Public Health), and Halsted Holman (Stanford University). For background, the participants reviewed research developments in the fields of physics, medicine, and genetics that have arisen as a result of the development of the nuclear age. They stated their conviction that the scientist has a duty to communicate to the public the results of his research, in terms understandable to nonscientists, and his informed opinion on the significance to society of the scientific developments. They further discussed the scientist's role in the formulation of public policy. Agreement that the scientist has such a role was general, but opinions differed on how he should participate in policy-decision-making, ranging from the view that he should be completely involved, by running for political office or accepting

political appointments, to the view that he should remain quietly in the laboratory until asked for his advice on a specific subject.

The need for the scientist to communicate to the nonscientific public, and the manner in which this could be accomplished, were major topics in the ensuing discussion. Reporter Brown of the *Sacramento Bee* came to the defense of the newspapers after somewhat derogatory remarks had been directed against them. He felt that the scientist should approach this communication problem scientifically by learning more about the manner in which newspapers must operate, and that by so doing he would be better able to give the proper information to the reporters and much less critical of the way it came out in the newspapers. He believed that scientists were too much concerned with the opinion a colleague might draw from a newspaper article and not concerned enough with the problem of transmitting the essential "kernel" of information to the general public.

The more specific symposium, on biological implications of radioactive isotopes in the sea, was organized and chaired for ASLO by William Aron, of the University of Washington. William Royce (Fisheries Research Institute, University of Washington) discussed the rapid increase in the use of food from the sea, pointing out that not only may radioactive pollution be harmful but that the mere suspicion of harmful effects can create international problems of high propaganda potential. B. H. Ketchum (Woods Hole Oceanographic Institution) showed that marine organisms, by concentrating chemical elements and by horizontal and vertical migration, could cause as much transport of radioactive isotopes upward or downward as the physical mixing processes in the ocean. From investigations at Rongelap Atoll, Edward Held (University of Washington) indicated the qualitative distribution of radionuclides in the flora and fauna 5 years after contamination from a single fallout event. There are distinct differences in concentration between the terrestrial and the marine environments, and levels in man reflect both the terrestrial and the marine sources of his food.

Data from monitoring low-level radioactive wastes flowing into the Irish Sea from the British reactor at Windscale were reviewed by Michael Waldichuk (Fisheries Research Board of Canada Biological Laboratory, Na-

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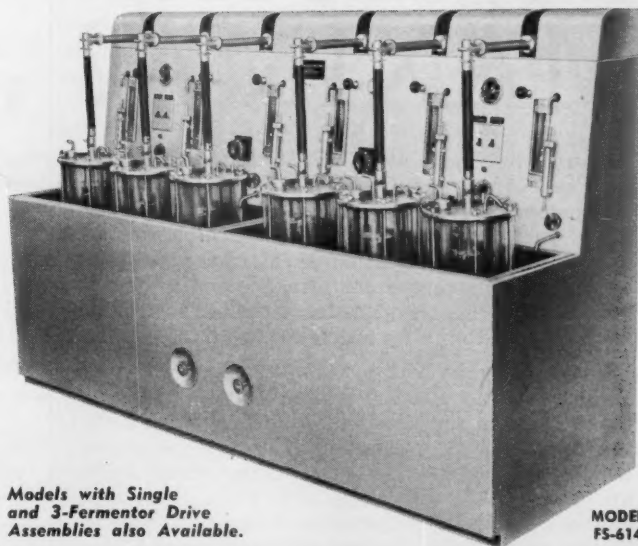
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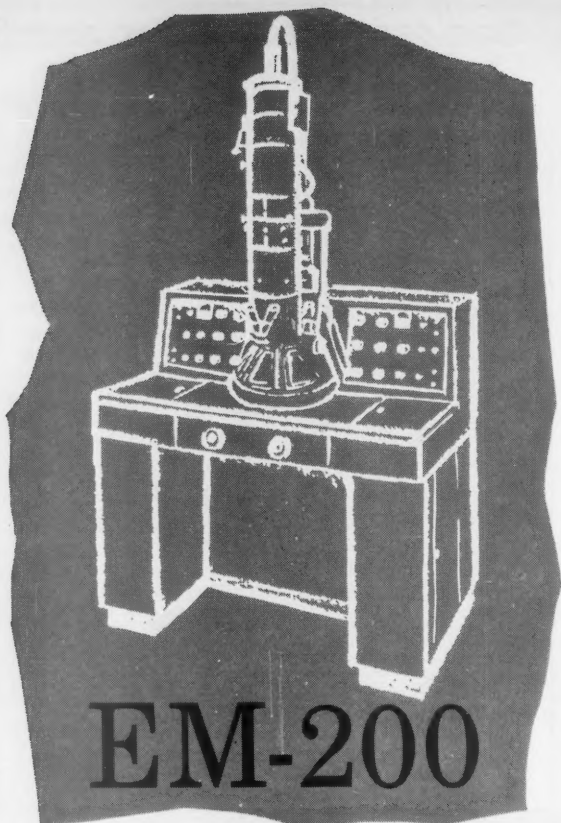
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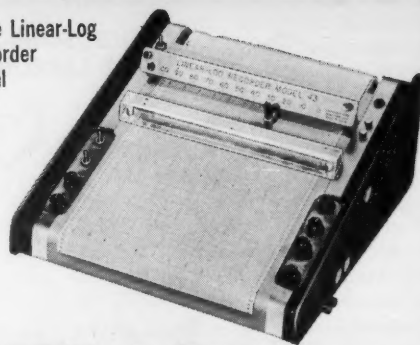
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naimo, British Columbia) as a basis for summarizing the oceanographic problems encountered in attempting to predict levels of radioactive pollution. Joel Hedgpeth (Pacific Marine Station, Dillon Beach, California), well known exponent of the dangers of disturbing an ecological balance, surprised the group by conceding that some radioactive pollution in the sea appears to be inevitable, and noted that a certain amount of radiation background may even be essential to life. However, he stressed that we should learn much more about the effects on the biota itself, as distinct from use of the biota by man, before tampering very much with the environment. O. E. Sette (Bureau of Commercial Fisheries Biological Laboratory, Stanford, California), in summarizing and correlating the highlights of the talks, pointed out that there may be one quite beneficial side effect of the radioactive pollution problem—the stimulation of many phases of instrumentation and research in oceanography, especially of research on interrelations in the biological food web in the sea.

J. F. T. SAUR

Bureau of Commercial Fisheries,  
U.S. Fish and Wildlife Service,  
Stanford, California

## Forthcoming Events

### October

23-24. Institute of the Aerospace Sciences and the Canadian Aeronautical Inst., Ottawa, Ont., Canada. (H. Harris, IAS, 2 E. 64 St., New York 21)

23-28. Congress of Chemical Engineering, 1st, San Juan, P.R. (R. Munoz, Apartado 47, Estación de Río Piedras, San Juan)

24-25. Shallow Water Research Conf., Gulf Coast, 1st natl., Tallahassee, Fla. (D. S. Gorsline, Oceanographic Inst., Florida State Univ., Tallahassee)

24-26. Aerospace Nuclear Propulsion, intern. symp., Las Vegas, Nev. (P. M. Uthe, Lawrence Radiation Laboratory, Univ. of California, Box 808, Livermore)

24-27. American Dietetic Assoc., 44th annual, St. Louis, Mo. (Mrs. T. Pollen, ADA, 620 N. Michigan Ave., Chicago 11, Ill.)

26-27. American Soc. of Tool and Manufacturing Engineers, Toronto, Canada. (A. Cervenka, Vanderbilt Blvd., Oakdale, L.I., N.Y.)

26-27. Instrumentation - Facilities for Biomedical Research, symp., Omaha, Neb. (H. G. Beenken, Univ. of Nebraska College of Medicine, 42 and Dewey Ave., Omaha)

26-27. New Mexico Acad. of Science, Albuquerque. (K. G. Melgaard, P.O. Box 546, Mesilla Park, N.M.)

26-28. Professional Group on Electron

Devices, annual meeting, Washington, D.C. (I. M. Ross, Technical Program Chairman, Room 2A-329, Bell Telephone Laboratories, Murray Hill, N.J.)

26-30. American Soc. for Aesthetics, Detroit, Mich. (J. R. Johnson, Cleveland Museum of Art, Cleveland 6, Ohio)

27-28. Shallow Water Research Conf., Pacific Coast, 1st natl., Los Angeles, Calif. (D. S. Gorsline, Oceanographic Inst., Florida State Univ., Tallahassee)

27-29. Association of Clinical Scientists, annual, Washington, D.C. (R. P. MacFate, Secretary, ACS, 323 Northwood Rd., Riverside, Ill.)

28. American Mathematical Soc., 583rd meeting, Cambridge, Mass. (E. Pitcher, Lehigh Univ., Bethlehem, Pa.)

29-31. Photoelasticity, intern. symp., Chicago, Ill. (P. D. Flynn, Illinois Inst. of Technology, Chicago 16)

29-1. Marine Biology, intern. conf. (by invitation only), Princeton, N.J. (Mrs. E. Purcell, Interdisciplinary Conference Program, Rockefeller Center, Time & Life Bldg., New York 20)

30-1. American Oil Chemists Soc., Chicago, Ill. (W. O. Lundberg, Hormel Inst., Univ. of Minnesota, 801 16th Ave., NE, Austin)

30-1. Society of Rheology, annual, Madison, Wis. (J. D. Ferry, Univ. of Wisconsin, Madison)

31-2. Interscience Conf. on Antimicrobial Agents and Chemotherapy, 1st, American Soc. for Microbiology, New York, N.Y. (ASM, 19875 Mack Ave., Detroit 36, Mich.)

### November

1. Rheumatic Fever, symp., New Haven, Conn. (E. A. Sillman, Connecticut Heart Assoc., 65 Wethersfield Ave., Hartford 14, Conn.)

1-3. Alkaline Pulping, 15th conf., Houston, Tex. (Technical Assoc. of the Pulp and Paper Industry, 360 Lexington Ave., New York 17)

1-3. Experimental Mechanics, 1st intern. congr., New York, N.Y. (Soc. for Experimental Stress Analysis, P.O. Box



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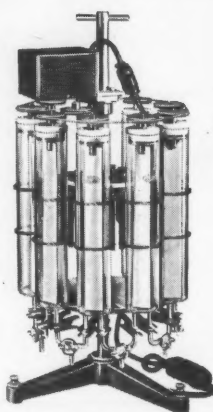
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1-3. High Magnetic Fields, intern. conf., Cambridge, Mass. (H. H. Kolm, Lincoln Laboratory, Massachusetts Inst. of Technology, Lexington 73)

1-3. Transplantation, CIBA Foundation symp. (by invitation), London, England. (CIBA Foundation, 41 Portland Pl., London, W.1)

1-4. American Soc. of Tropical Medicine and Hygiene, Washington, D.C. (R. B. Hill, 3575 St. Gaudens Rd., Miami 33, Fla.)

1-4. Society of Economic Geologists, Cincinnati, Ohio. (E. N. Cameron, Science Hall, Univ. of Wisconsin, Madison 8)

2-3. Cancer Chemotherapy, clinical symp., Washington, D.C. (T. P. Waalkes, Chemotherapy Natl. Service Center, NIH, Bethesda 14, Md.)

2-4. American Soc. for Cell Biology, 1st, Chicago, Ill. (H. Swift, Dept. of Zoology, Univ. of Chicago, Chicago 37)

2-4. Geochemical Soc., Cincinnati, Ohio. (F. R. Boyd, Jr., Geophysical Laboratory, 2801 Upton St., NW, Washington 8)

2-4. Geological Soc. of America, Cincinnati, Ohio. (F. Betz, Jr., GSA, 419 W. 117 St., New York 27)

2-4. Inter-Society Cytology Council, annual, Memphis, Tenn. (P. A. Young, 1101 Beacon St., Brookline 46, Mass.)

2-4. National Assoc. of Geology Teachers, Cincinnati, Ohio. (D. J. Gare, Principia College, Elsah, Ill.)

2-4. Paleontological Soc., Cincinnati, Ohio. (H. B. Whittington, MCZ, Harvard Univ., Cambridge 38, Mass.)

2-4. Society for Industrial and Applied Mathematics, Washington, D.C. (Chairman, Program Committee, SIAM, P.O. Box 7541, Philadelphia 1, Pa.)

2-5. Mathematical Models in the Social and Behavioral Sciences, conf., Cambria, Calif. (F. Massarik or P. Ratoosh, Mathematical Models Conf., Graduate School of Business Administration, Univ. of California, Los Angeles 24)

3-4. Central Soc. for Clinical Research, Chicago Ill. (J. F. Hammarsten, Veterans Administration Hospital, 921 N.E. 13 St., Oklahoma City 4, Okla.)

4. Society for the Scientific Study of Sex, New York, N.Y. (H. G. Beigel, 139 E. 94 St., New York 28)

5-8. American Speech and Hearing Assoc., Chicago, Ill. (K. O. Johnson, 1001 Connecticut Ave., NW, Washington 6)

5-9. Society of Exploration Geophysicists, 31st annual intern., Denver, Colo. (C. C. Campbell, Box 1536, Tulsa 1, Okla.)

5-11. Stomatology of Peru, intern. congr., Lima, Peru. (A. Rojas, Avenue Pershing 155, San Isidro, Lima)

5-15. Japanese Chemical Engineers Soc. 25th anniversary congr., Tokyo and Kyoto, Japan. (Kagaku-Kogaku Kyokai, Shun-ichi Uchida, 609 Kojunsha Bldg. No. 4, 6-Chome, Ginza, Chou-Ku, Tokyo)

5-18. Latin American Phytotechnical Meeting, 5th, Buenos Aires, Argentina. (U. C. Garcia, Rivadavia 1439, Buenos Aires)

6-8. Association of Military Surgeons of the U.S., 68th annual, Washington, D.C. (R. E. Bitner, AMSUS, 1726 Eye St., NW, Washington 6)

6-8. Cell in Mitosis, 1st annual symp.

Detroit, Mich. (L. Levine, Dept. of Biology, Life Sciences Research Center, Wayne State Univ., Detroit 2)

6-8. Chemical Engineering Div., Chemical Inst. of Canada, Toronto, Ont. (CIC, 48 Rideau St., Ottawa 2, Ont.)

6-9. Atomic Industrial Forum-9th Hot Laboratories and Equipment Conf., Chicago, Ill. (O. J. Du Temple, American Nuclear Soc., 86 E. Randolph St., Chicago)

6-9. Southern Medical Assoc., Dallas, Tex. (R. F. Butts, 2601 Highland Ave., Birmingham 5, Ala.)

8. American Acad. of Arts and Sciences, Brookline, Mass. (J. L. Oncley, 280 Newton St., Brookline 46)

8-10. Nondestructive Testing in Electrical Engineering, conf., London, England. (Secretary, Institution of Electrical Engineers, London W.C.2)

8-11. Acoustical Soc. of America, Cincinnati, Ohio. (W. Waterfall, American Inst. of Physics, 335 E. 45 St., New York 17)

8-11. Institute of Management Sciences, San Francisco, Calif. (W. Smith, Inst. of Science & Technology, Univ. of Michigan, Ann Arbor)

8-11. Plasma Physics, American Physical Soc., 3rd annual, Colorado Springs, Colo. (F. Ribe, Los Alamos Scientific Laboratory, P.O. Box 1663, Los Alamos, N.M.)

9-10. Operations Research Soc. of America, 20th, San Francisco, Calif. (P. Stillson, 115 Grove Lane, Walnut Creek, Calif.)

9-11. Gerontological Soc., Pittsburgh, Pa. (R. W. Kleemeier, Washington Univ., Skinker and Lindell, St. Louis 30, Mo.)

9-12. Pacific Coast Fertility Soc., Palm Springs, Calif. (G. Smith, 909 Hyde St., San Francisco 9, Calif.)

9-20. Photography, Cinematography, and Optics, 3rd intern. biennial, Paris, France. (Comité Français des Expositions, 15 rue de Bellechasse, Paris 7)

12-17. Bahamas Conf. on Medical and Biological Problems in Space Flight, Nassau, Bahamas. (I. M. Wechsler, P.O. Box 1454, Nassau)

13-14. Exploding Wire Phenomenon, 2nd intern. conf., Boston, Mass. (W. G. Chace, Thermal Radiation Laboratory, CRZCM, Geophysics Research Directorate, Air Force Cambridge Research Laboratories, Bedford, Mass.)

13-16. Magnetism and Magnetic Materials, 7th annual intern. conf., Phoenix, Ariz. (P. B. Myers, Motorola, Inc., 5005 E. McDowell Rd., Phoenix 10)

13-17. American Public Health Assoc., 89th annual, New York, N.Y. (APHA, 1790 Broadway, New York)

13-17. Gulf and Caribbean Fisheries Inst., 14th annual, Miami Beach, Fla. (J. B. Higman, Marine Laboratory, Univ. of Miami, 1 Rickenbacker Causeway, Virginia Key, Miami 49)

13-18. European Conf. on the Control of Communicable Eye Diseases, Istanbul, Turkey. (World Health Organization, Palais des Nations, Geneva, Switzerland)

14-16. American Meteorological Soc., Tallahassee, Fla. (Executive Secretary, AMS, 45 Beacon St., Boston 8, Mass.)

14-17. Corrosion in Nuclear Technology, symp., Paris, France. (European Federation of Corrosion, Société de Chimie

Industrielle, 28 rue St. Dominique, Paris 7<sup>e</sup>)

14-18. Puerto Rico Medical Assoc., Santurce. (J. A. Sanchez, P.O. Box 9111, Santurce)

15-17. Eastern Analytical Symp., New York, N.Y. (A. Rekus, EAS, Research Dept., Baltimore Gas & Electric Co., Pratt St., Baltimore, Md.)

15-18. Society of Naval Architects and Marine Engineers, annual, New York, N.Y. (W. N. Landers, SNAME, 74 Trinity Pl., New York 6)

16-18. American Psychiatric Assoc., Milwaukee, Wis. (J. D. McGucken, 756 N. Milwaukee St., Milwaukee 2)

16-18. Etiology of Myocardial Infarction, intern. symp. (by invitation), Detroit, Mich. (T. N. James, Section on Cardiovascular Research, Henry Ford Hospital, Detroit)

16-18. Southern Thoracic Surgical Assoc., Memphis, Tenn. (H. H. Seiler, 517 Bayshore, Blvd., Tampa 6, Fla.)

16-19. American Anthropological Assoc., Philadelphia, Pa. (S. T. Boggs, 1530 P St., NW, Washington, D.C.)

17-18. Southern Soc. for Pediatric Research, Atlanta, Ga. (W. G. Thurman, Dept. of Pediatrics, Emory Univ. School of Medicine, Atlanta)

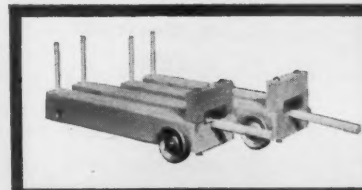
17-31. National Soc. for Crippled Chil-

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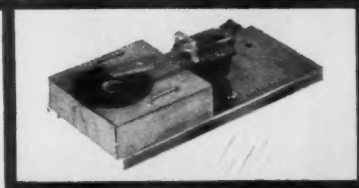
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19-22. International College of Surgeons, Western regional, San Francisco, Calif. (W. F. James, 1516 Lake Shore Drive, Chicago 10, Ill.)

22-27. Automation and Instrumentation, 5th conf., Milan, Italy. (Federezazione delle Societa Scientifiche e Tecniche di Milano, via S. Tomaso 3, Milan)

22-1. Radioisotopes in Animal Biology and the Medical Sciences, conf., Mexico City, D.F. (International Atomic Energy Agency, 11 Kärntner Ring, Vienna 1, Austria)

## Letters

### Coconut Water

I would like to make a suggestion on coconut (*Cocos nucifera*, L.) terminology. Many scientists have been attracted to the use of coconut liquid endosperm because of its nutritive properties for plant tissue-culture work and sporulation of fungi. Readers are somewhat confused with the terms used

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In most Asian countries the term *coconut milk* refers to the milky-white sap expressed from grated nut meat or solid endosperm, *coconut water* to the liquid endosperm. Many more uses of the latter are likely to develop. I therefore suggest, for uniformity in terminology, that only one term—*coconut water*—be used to refer to the liquid endosperm, in order to avoid confusion.

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College of Agriculture, University of the Philippines, College, Laguna

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### Modern Biology

I began Commoner's article "In defense of biology" [*Science* 133, 1745, 1961] in the peace and quiet, almost the somnolence, of a comfortable armchair and it wasn't until the bottom of the second column that it broke upon me that biology was being defended against none other than myself. Commoner is concerned, it seems, over the attitude taken toward biology in my book, *The Intelligent Man's Guide to Science*, and, in particular, is horrified at my statement that "modern science has all but wiped out the border-line between life and non-life."

In response, Commoner says: "Since biology is the science of life, any successful obliteration of the distinction between living things and other forms of matter ends forever the usefulness of biology as a separate science. If the foregoing sentence is even remotely correct, biology is not only under attack; it has been annihilated."

I could not help but be moved by the anguish clearly detectable in this *cri de coeur*, and I long to assure Commoner that he need not fear. Biology will not be annihilated even if the boundary between life and nonlife vanishes.

There was, after all, a time when astronomical advance removed the boundary between earth and the other planets, and that did not annihilate geology as a separate science. The advance of knowledge in biology removed the boundary between man and other species, and that has not annihilated sociology as a separate science. In fact, both geology and sociology became more meaningful when both could draw upon and, in turn, enlighten, a broader field of inquiry.

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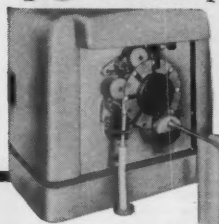
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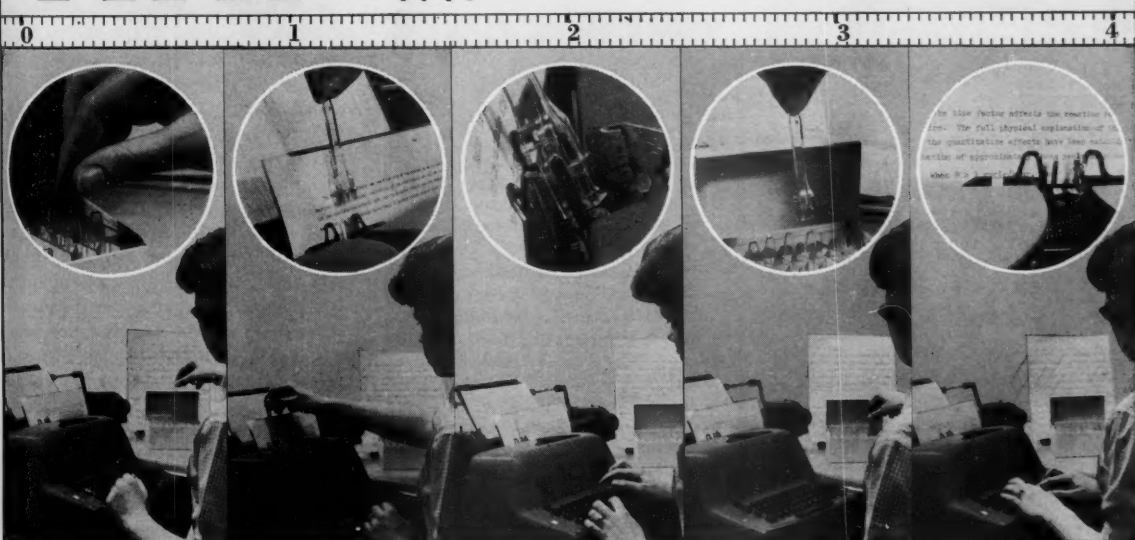
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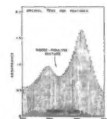
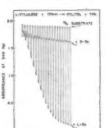
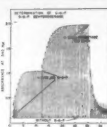
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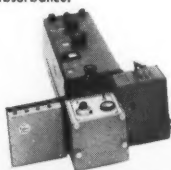
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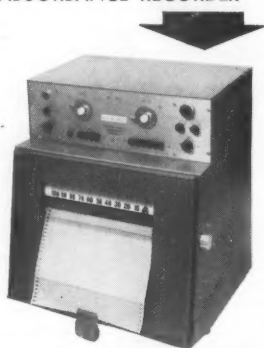
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If the distinction between life and nonlife vanishes, the science of biology will persist as the study of a particular collection of material systems that we will still call "life" for simplicity's sake, just as we still speak of "organic chemistry" more than a hundred years after the distinction between organic chemistry and inorganic chemistry vanished.

In fact, the importance of biology will be heightened when physicists and chemists come to realize that biology's deepest insights will be of direct service to their own fields of specialization.

Science is a unit, and if it seems broken up into arbitrary divisions, that is the fault of the age of intellectual overspecialization in which we live. Scientists who labor to make these artificial partitions between the arbitrary divisions impenetrable and unsurmountable are doing science a great disservice. Nor are they truly serving their own fields by their careful shielding of them from all contact with outside thought.

Commoner is also annoyed at my statement: "All of the substances of living matter—enzymes and all the others, whose production is catalyzed by enzymes—depend in the last analysis on DNA."

His statement, opposing that, is: "life is unique and . . . it cannot be reduced to the property of a single substance or of a system less complex than a living cell."

With reference to this remark (which he labels his "chief argument"), I can only applaud his courage, not his judgment, in declaring anything at all to be unique. There was a time when the earth was considered unique, as the only motionless object in the universe; when each species was a unique creation; when the organic chemical was uniquely a product of life, and the "organized ferment," uniquely a creature of the intact cell. Where are these uniquenesses and a hundred others? The history of science is filled with the bleached bones of uniqueness, dead at the thrust of knowledge that has become more and more comprehensive.

As to Commoner's remark that life cannot be reduced to the property of a single substance or of a system less complex than a living cell (and with what courage he pronounces his negative fiat), I repeat my own remark that life depends, in the last analysis, on DNA.

To depend "in the last analysis" does not necessarily imply a simple or direct dependence. The dependence is a supercomplex one, in fact, but that in itself does not alter matters.



life and biology particular that we have's sake, chemists after chemists. biology will d chem- deepest to their seems ns, that ellectual ve live. ese arti- rbitrary rmount- disserv- their own of them ought. at my s of liv- e others. by en- lysis on is: "life reduced tance or a living (which ), I can his judg- all to be he earth only mo- e; when creation: uniquely rganized of the in- uenesses story of ed bones thrust of ore and that life erty of a ess com- ith what negative that life a DNA. sis" does or direct a super- in itself

One can say that American law is based, *in the last analysis*, on the Constitution, but that does not mean that any amount of reading of the Constitution *alone* will explain the nature of the city ordinances of Tulsa, Oklahoma. For that matter, a close reading of the 14th Amendment will not elucidate the social structure of Mississippi. Yet it remains fair to say that the Constitution is the basic law of the land.

And life depends, in the last analysis, on DNA.

If Commoner disapproves of the incoming tide and wishes to amuse himself by standing on the shore and commanding it to stop, he may. He may also quote as many authorities as he likes in order to impress the waves.

But he will get his feet wet just the same.

ISAAC ASIMOV

Boston University School of Medicine,  
Boston, Massachusetts

Barry Commoner is to be congratulated on his succinct and elegant article on the status of "traditional" and "modern" biology, and on his proposal to restore the science of life. If one assumes general agreement on this goal, the question arises, "How is one to proceed?" I believe that things might be initiated if all modern biologists were asked to take three giant steps backwards and to ask themselves the question: "What is really being studied when we study life?"

Perhaps the chief factor which has placed modern biology in a situation analogous to that in which 19th-century physics found itself in the first part of this century is that modern biologists have failed to realize that the study of life is the study of living organisms and living cells. As Commoner has pointed out, Bohr's theory of complementarity should set the limits as to how far one can go in biology, just as Heisenberg's principle of indeterminacy sets obvious limits in physics. It seems almost axiomatic that putting physicochemical questions to a physical-chemical system results in answers only in terms of the units used, and not in terms of the holistic nature of the organism or intact cell. Furthermore, the fact of epigenesis, and the apparent requirement of DNA for a cellular environment to execute its action, should put into more proper perspective the tingling sensation and fascination which arises at the mention of DNA. The imbalance of interest in DNA certainly stems from such statements as Asimov's that "modern science

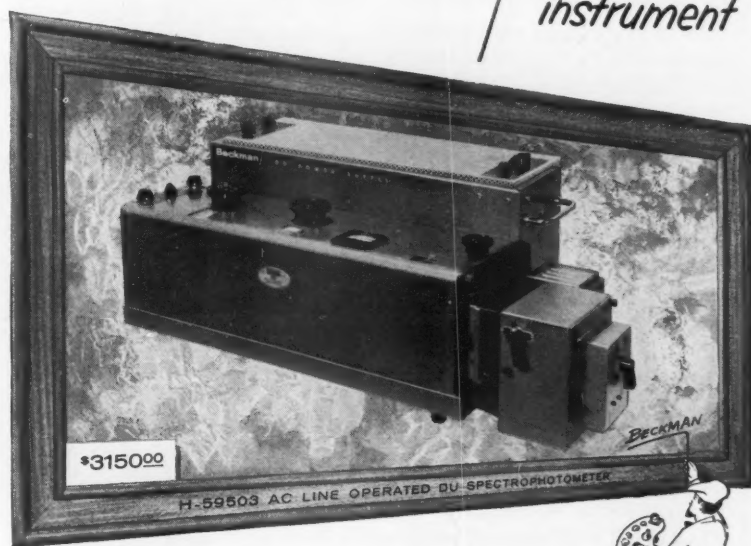
has all but wiped out the border-line between life and non-life." Such a statement has accuracy only within the framework of what one considers life to be. Replication of molecules is not life.

The regrettable schism between "breakthrough" areas and traditional areas of biology has resulted in a state of affairs that needs obvious correction. Some examples from my personal experience come to mind: (i) ridicule of the direct observational method and a predilection for indirect methods; (ii)

introductory courses given over completely to nontraditional biology; (iii) a course in protozoology devoted entirely to a single species; (iv) a professor telling his freshman students that remembering names of species and classification was a discredited method and was not required in his course (he then went on to have them memorize the Krebs cycle, which is really a kind of classification); (v) an internationally known modern biologist, who, having given a seminar on nucleoproteins of frogs, was

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asked what species of frog was used; his answer, "I don't know, they were bought from some supply house"; (vi) a pre-doctoral graduate student in biophysics, supported by a large government stipend, declaring that his only wish was to see a protein molecule divide under a microscope (light); (vii) the teaching in many universities that viruses are organisms and bridge the gap between living and nonliving phenomena; (viii) financial support for "breakthrough" areas rather than additive areas of varying magnitude; and (ix) obsession with confidence limits and evaluations. I am certain that any reader can compile an even longer list.

We might learn a lesson from physics, where the mother-child relationship has not been so prodigal. The wave and corpuscular theories of light are still used and found necessary to explain certain phenomena, notwithstanding the quantum theory. I think Commoner has done a great service to all biologists in making a plea for the integrity of traditional and modern disciplines. Furthermore, it is my belief that the bewildered mother would receive again her fast-talking child, and would even take on

some of the child's habits, should the road be made open and should agreement be reached as to what each was examining.

H. H. NAJARIAN  
University of Texas Medical Branch,  
Galveston

I believe that most of Asimov's remarks require no comment beyond what has already been said in my original article. However, one of his statements tends to give the reader a misleading idea of the content of my article, which I should like to correct.

With respect to the distinctions between living and other forms of matter, Asimov states: "Scientists who labor to make these artificial partitions between the arbitrary divisions impenetrable and unmountable are doing science a great disservice. Nor are they truly serving their own fields by their careful shielding of them from all contact with outside thought."

In the second of the foregoing sentences Asimov has precisely reversed the main point of my article—which is that the real distinction between life and

nonlife ought to be recognized so that physics and chemistry can be more effectively applied to biological problems. Far from avoiding "contact with outside thought," my article consists of arguments in support of the uniqueness of life which are derived from physical and chemical principles.

In the first of the quoted sentences, Asimov has also misinterpreted that part of my article which deals with the significance, for biology, of the principle of complementarity put forward by Bohr. Bohr points out that for reasons which are fundamental to the present structure of quantum physics, a subatomic particle can be described, at any one time, by either its corpuscular or its wave properties, but never by both together. He suggests—and I support this view—that a similar complementarity governs the relationship between two features exhibited by living organisms—the manifest living state and the physicochemical events which go on within it. Asimov is at liberty to regard this proposal as an "artificial partition between the arbitrary divisions" of life and nonlife. But in that case he ought, in the name of logic, to say the same about the distinction between the corpuscular and wave properties of subatomic particles. Yet, much of modern physics has been founded on this distinction.

BARRY COMMONER  
Committee on Molecular Biology,  
Adolphus Busch III Laboratory,  
Washington University,  
St. Louis, Missouri

### Daedalus and Minos

Dedijer's very interesting article "Why did Daedalus leave?" [*Science* 133, 2047 (1961)] leaves open a tantalizing question: How about the fate of Minos? In those countries in which the social and political environment is unfavorable to the development of science, does not the return of the Daedali deeply threaten the social system which led to their flight? Would not their return and creation of new traditions necessary to their existence construct a bath to scald Minos to death? And is not Minos, unconsciously perhaps, behaving in his own self-interest in putting last on his priority list "a quantity of problemsolving Daedali?"

LINDSEY R. HARMON  
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## IMMUNOCHEMICAL APPROACHES TO PROBLEMS IN MICROBIOLOGY

Edited by Michael Heidelberger and Otto J. Plescia,  
Institute of Microbiology, Rutgers, The State University.

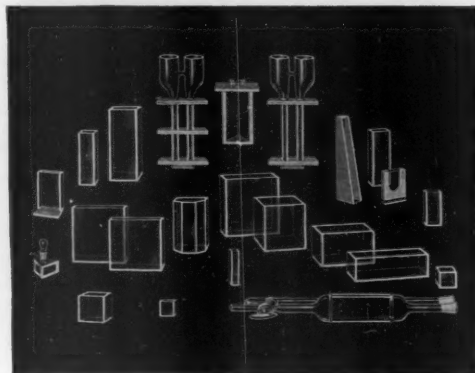
A volume based on a recent Symposium which brought together scientists from different disciplines to consider more effective utilization of immunochemical methods in the further development of microbiology. The papers and discussions by panelists and invited audience cover the topics: Quantitative immunochemical aspects of microbial specificity; precipitation and inhibition techniques, multiplicity and specificity of bacterial antigens, immunochemistry of viruses, so-called non-specific factors in immunity, and biosynthesis of antigens and antibodies.

The panelists were Ö. Ouchterlony, Pierre Grabar, H. G. Kunkel, J. R. Marrack, Paul H. Maurer, E. A. Kabat, J. Baddiley, Maclyn McCarty, James T. Park, D. L. Shrivastava, C. A. Knight, Lawrence Levine, Igor Tamm, Fred Karush, Raymond Latarjet, O. G. Bier, Elmer L. Becker, Otto J. Plescia, Manfred M. Mayer, Irwin H. Lepow, Robert A. Nelson, Jr., Myron A. Leon, Evelyn E. B. Smith, George T. Mills, Brigitte A. Askonas, Felix Haurowitz, G. J. V. Nossal, and O. Mäkelä.

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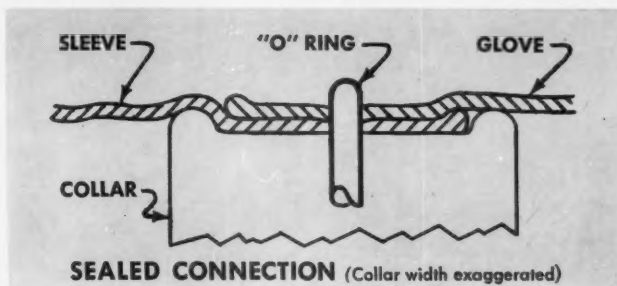
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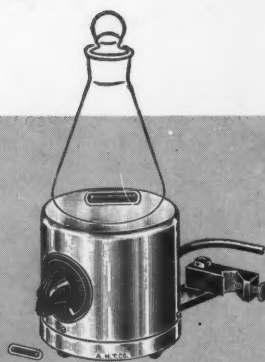
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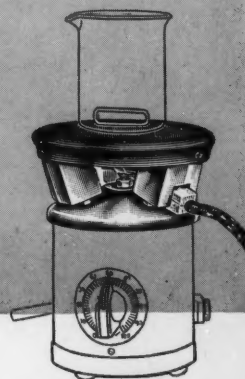
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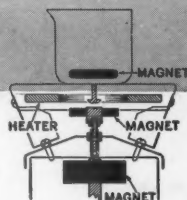
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9235-C.  
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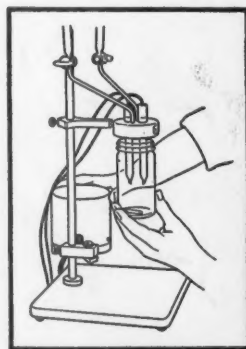
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